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UNITED STATES DISTRICT COURT
SOUTHERN DISTRICT OF TEXAS
CORPUS CHRISTI DIVISION

MARC VEASEY, *et al*,	§	
	§	
Plaintiffs,	§	
VS.	§	CIVIL ACTION NO. 2:13-CV-00193
	§	
RICK PERRY, *et al*,	§	
	§	
Defendants.	§	

SUPPLEMENTAL REBUTTAL DECLARATION OF JEFFREY MILYO

I, Jeffrey Milyo, do hereby declare the following:

I. Statement of Inquiry:

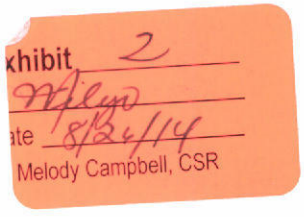
1. I have been asked by the State of Texas to review the expert reports submitted on behalf of plaintiffs and to comment on the quality of the arguments made regarding the recently implemented voter identification law in Texas (SB 14).

2. I have received and reviewed a total of seventeen expert reports that were submitted by the plaintiffs. These are the primary materials that I rely upon in conducting this review. In some instances, I also examine data sets and scholarly literature used or cited by these experts. I also rely on my knowledge of the relevant scholarly literature; all outside sources that I employ are cited in this report.

3. Given the limited time available to me, this review is not intended to be comprehensive or exhaustive. For this reason, I select what are (in my opinion and given my expertise) some of the important weaknesses in the arguments and evidence presented by the experts for the plaintiffs. As a further consequence, I may amend or revise this report as additional evidence and arguments come to my attention.

4. Also, as I was completing my report, I became aware that experts for the plaintiff may not have been provided with all relevant records from the Texas Department of Public Safety. However, the substance of my report has not been written with foreknowledge of this event. My understanding is that experts for the plaintiffs may revise their reports; so accordingly, I also reserve the right to amend and revise my analysis in response.

5. The State of Texas is compensating me for any subsequent testimony in this case at the rate of $400 per hour. To date, I have been paid $15,000 for my work in this case.



II. Qualifications:

6. I am a professor of economics and adjunct professor of political science at the University of Missouri in Columbia, Missouri. I was named the Middlebush Professor of Social Science at the University of Missouri (MU) for the academic years of 2008-2013. In addition, this fall I will be inducted into the 21st Century Corps of Discovery; this honor is given annually to one MU professor to highlight outstanding scholarship.[1] A current vita is appended to this report.

7. I received my Ph.D. in economics from Stanford University in 1994. In addition to my current faculty appointment at MU, I have served on the faculty in the economics and political science departments at Tufts University; the Harris School of Public Policy at the University of Chicago; and the Truman School of Public Affairs at the University of Missouri.

8. I have also been a visiting scholar in the political science department at Washington University in St. Louis, MO; the political science department at the Massachusetts Institute of Technology in Cambridge, MA; the Institute for Social and Policy Studies at Yale University in New Haven, CT; and both the Center for Economic Policy Research and the Hoover Institution at Stanford University in Stanford, CA. I have also been a non-residential fellow at the Safra Center for Ethics at Harvard University.

9. My area of expertise is American political economics and policy evaluation. My scholarly research has been published in several academic journals, including: the *American Economic Review*; the *American Journal of Public Health*; the *Election Law Journal*; the *Journal of Health, Politics, Policy and Law*; the *Journal of Human Resources*; the *Journal of Law and Economics*; the *Journal of Policy Analysis and Management*; the *Journal of Politics*; *Legislative Studies Quarterly*; *Public Choice*; the *Quarterly Journal of Economics*; *Social Science Quarterly*, and the *State Politics and Policy Quarterly*. I am also a frequent reviewer of submissions to these and several other academic journals.

10. I have taught a variety of undergraduate and graduate-level courses; these include courses on: American politics, law and economics, political economics, public economics, and public policy.

11. I am a senior fellow at the Cato Institution and an academic advisor the Center for Competitive Politics in Washington, DC. I recently served on the Election Reform Task Force for the Bipartisan Policy Center in Washington, DC. I also served as a member of the research staff for the Presidential Commission on Election Administration.

12. In 2007, I submitted written testimony on voter identification laws to the United States House Administration Committee; in 2008, I appeared as a witness in a public hearing on voter identification before the United States Senate Committee on Rules and Administration.

13. I have also produced expert reports in the following state and federal election law disputes:

[1] See: http://provost.missouri.edu/awards/campus-awards/corps.php.

(a) In 2003, I produced an expert report in an election dispute in the New Jersey state courts (*Re: The Contest of the Democratic Primary Election of June 3, 2003 for the Offices of Assembly and Senate, 31st Legislative District, Docket No. HUD L-3947-03 and HUD-L-3948-03 (Consolidated)*); my report described the impact of illegal campaign contributions on the outcome of several statehouse races in the Democratic primary in New Jersey.

(b) In 2006, I co-produced an expert report in a dispute over voter I.D. laws in the Missouri state courts (*Kathleen Weinschenk, et al. v. State of Missouri et al. and Jackson County, Missouri v. State of Missouri (Consolidated)*); my report described the number of legal voters that might be deterred from voting under Missouri's recently enacted (and subsequently overturned) law requiring photo identification at polling places.

(c) In 2006, I produced an expert report in a dispute over monetary damages in the New Hampshire state courts (*Buckley, et al. v. New Hampshire Republican State Committee, et al.*); my report was in regard to the amount of damages resulting from the illegal jamming of several phones in the headquarters of the New Hampshire Democratic Party on Election Day in November 2002.

(d) In 2007, I produced an expert report and was deposed in a dispute in federal district court that involved state campaign finance disclosure laws in ballot measure elections in Colorado (*Sampson v. Coffman*); my report examined the ability of ordinary citizens to comply with Colorado's disclosure requirements for issue committees.

(e) In 2008, I produced an expert report and was deposed in a dispute in federal court over federal contribution limits for groups that make independent expenditures (*SpeechNow.Org v. FEC*); my report examined the impact of contribution limits on the ability of independent citizen groups like SpeechNow to raise funds for the purpose of making independent expenditures.

(f) In 2011, I produced a report and was deposed in a dispute in federal court over Washington State's disclosure requirements for groups engaged in grass roots issue advocacy (*Many Cultures, One Message, et al., v. Clements, et al.*); my report analyzed the impact of Washington's Grass Roots Lobbying Law (Wash. Rev. Code § 42.17.200) on the ability of citizens to freely exercise their First Amendment rights to speak, associate, assemble and petition government.

III. Summary of Findings:

14. Experts for the plaintiffs assert than more than one million otherwise eligible voters in Texas lack SB 14 ID (or about 9% of registered voters). I demonstrate that these estimates are highly exaggerated. Moreover, I demonstrate that the methods employed by experts for the plaintiffs are expected to generate a large upward bias in the estimated number of eligible voters without ID. Given this, the analyses proffered by experts for the plaintiffs relating to the number or percent of voters who lack SB 14 ID are unreliable and misleading.

15. Experts for the plaintiffs assert that black and Hispanic voters in Texas are significantly more likely to be without requisite voter ID under SB 14. These estimated differences vary considerably across

experts; however, I demonstrate that all of these estimates are highly exaggerated. Moreover, I demonstrate that the methods employed by experts for the plaintiffs are expected to yield exaggerated estimates of the differences in rates of ID possession by race and ethnicity. In contrast, my re-examination of the survey data reported by Barreto and Sanchez reveals no statistically significant difference in rates of ID possession by race or ethnicity. Consequently, the analyses proffered by experts for the plaintiffs relating to the disproportionate number of black and Hispanic voters who lack requisite ID under SB 14 ID are unreliable and misleading.

16. Experts for the plaintiffs assert that there are substantial costs of obtaining voter identification and that these costs are particularly burdensome for black and Hispanic voters in Texas. However, these claims ignore the ability of individuals to economize such costs as well as the potential for outside assistance. Further, the costs of obtaining voter ID should be apportioned over multiple years and elections. Consequently, experts for the plaintiff greatly exaggerate the net costs of obtaining ID. Moreover, the estimated travel costs to obtain a free EIC are found to be higher for white voters versus black voters.

17. Experts for the plaintiffs argue that a small increase in the cost of voting from SB 14 will have the effect of suppressing turnout among blacks and Hispanics in Texas. However, the primary support for these claims is a defunct theory of voting from more than 50 years ago. I demonstrate that the theoretical effects of SB 14 on turnout are ambiguous, so that the claims regarding turnout can only be evaluated empirically. However, the most relevant empirical literature on the effects of voter ID laws and turnout provides no strong or consistent support for these claims. Instead, recent evidence suggests that state voter ID laws may have a mobilizing effect on voter turnout, even or especially among minority voters. The experts for the plaintiffs fail to acknowledge this directly relevant scholarly literature. Experts for the plaintiffs also fail to conduct any systematic statistical analysis of the treatment effects of state voter ID laws on turnout in their reports. For example, there have been several elections since the implementation of SB 14 in Texas; these elections provide a "natural experiment" for analyzing the impact of SB 14 on voter turnout. Taken together, these failures call into question the reliability of the experts for the plaintiffs.

IV. Introduction:

18. In my opinion, the arguments presented in the seventeen expert reports submitted by plaintiffs may be summarized as follows:

(a) *Voters without ID:* About 1.2 million eligible voters in Texas do not possess the types of identification required to cast a regular ballot at the polls under SB 14.

(b) *Differences in the Possession of ID:* Otherwise eligible black and Hispanic voters in Texas are significantly less likely to possess the types of identification required to cast a regular ballot at the polls under SB 14.

(c) *Costs of Obtaining ID:* The costs associated with meeting the identification requirements in SB 14 are substantial and these costs are especially burdensome for otherwise voting-eligible black and Hispanic citizens of Texas.

(d) *Voter ID as an Obstacle to Voting:* SB 14 will have the effect of substantially and disproportionately suppressing turnout among otherwise eligible black and Hispanic voters.

(e) *Racially Discriminatory Intent:* SB 14 was passed with the intent to substantially and disproportionately suppress turnout among otherwise eligible black and Hispanic voters.

Given the limited time available to me, I consider only the first four of these arguments (a-d). However, my findings are sufficient to severely undercut the fifth and final argument (e).

V. Voters without ID

19. Experts for the plaintiffs estimate the number of persons without acceptable identification under SB 14. Any such estimate involves some degree of uncertainty and is subject to bias. I demonstrate in this section that experts for the plaintiffs do not sufficiently acknowledge the uncertainty and bias in their estimates and do not take sufficient care to reduce bias in their estimates. Consequently, the estimates of the number or percentage of voters in Texas who lack requisite ID proffered by experts for the plaintiff are overstated and unreliable.

20. Experts for the plaintiffs employ two different methods for estimating the number of voters who lack requisite ID under SB 14; the first involves database matching and the second is based on an opinion survey. I consider each of these methods in turn.

Database Matching Method

21. One method by which the number of persons without acceptable identification may be estimated is by comparing the names of persons contained in the Texas Election Administration Management (TEAM) database to the names of persons listed in other state and federal databases. This matching process is described in detail in the expert report submitted by Dr. Ansolabehere. However, I am unaware of any scholarly studies that analyze the effects of voter ID by examining "non-matches" between a state voter registration database and external databases, as done by experts for the plaintiffs. There are several problems with this approach, some of which I describe below.

22. It is well known that state registration databases contain errors and that in general counts from such databases exaggerate the actual number of eligible and currently registered voters. For example, in his expert report for the plaintiffs, Dr. Ansolabehere notes that "All states that have voter registration will have at least some registrations that are out-of-date or invalid but still on the rolls, for a wide variety of reasons."[2] It is for this reason that much of the scholarly research on voter turnout in the United States eschews measures of turnout as a percent of registered voters and instead examines turnout relative to voting age population (VAP), citizen voting age population (CVAP), or voting eligible

[2] Report by Stephen Ansolabehere, p. 37.

population (VEP).[3] I am unaware of any scholarly studies that attempt to estimate the effects of voter ID by matching records to a state voter registration database.

23. The number of deadwood records in state voter registration databases is generally understood to be large.[4] For example, one recent study found that official voter registration records imply that about 81.2% CVAP are registered to vote, while self-reports from the Current Population Survey (CPS) suggest only 72.9% of CVAP are registered to vote.[5] This alone suggests that more than 10% of voter registration records are deadwood.[6] Even so, this figure likely understates the percent of deadwood in state voter registration databases.

24. The CPS estimates the number of registered voters by summing the number of respondents that self-report voting and the number of non-voters who self-report being registered to vote.[7] However, it is well-known that survey responses generally exaggerate socially desirable behavior, such as registering and voting.[8] Further, one recent study finds that over-reports of voting in the CPS are highest in Southern states and in states with more minority population.[9] Moreover, this same study finds that self-reported turnout in the CPS exceeds actual turnout in Texas by more than 14 percentage points.

25. For the sake of illustrating the potential magnitude of deadwood in state voter rolls, I will assume that the rate of over-reporting of registration in the CPS is similar to the rate of over-reporting of voter turnout.[10] Combining these over-reporting percentages in the studies cited above then implies that about 24% of registered voters in Texas may be deadwood.

26. To the extent that the state of Texas has improved the quality of its registration data in recent years, the actual percentage of deadwood in the TEAM database may well be lower than 24%, but the percentage is certainly not zero. For example, Dr. Ansolabehere has claimed that that there have been improvements in the quality of state voter registration records, but he still describes state registration

[3] For example, see McDonald, Michael P. and Samuel L. Popkin. 2002. "The Myth of the Vanishing Voter." *American Political Science Review*, 95(4): 963–74; and Burden, Barry. 2000. "Voter Turnout and the National Election Studies" Political Analysis, 8(4): 389-398.

[4] For ease of exposition, I will use the term "deadwood" to describe any name listed in a database of registered voters that is not a real person, not alive, not residing at that address, or otherwise not actually an eligible voter.

[5] McDonald, Michael P. 2007. "The True Electorate: A Cross-Validation of Voter Registration Files and Election Survey Demographics." *Public Opinion Quarterly*, 71(4): 588–602.

[6] [100% * (81.2 – 72.9)/81.2].

[7] "About Voting and Registration" at http://www.census.gov/hhes/www/socdemo/voting/about/index.html (last viewed July 16, 2014).

[8] E.g., Holbrook, Allyson L. and Jon A. Krosnick 2010. "Social Desirability Bias in Voter Turnout Reports: Tests Using the Item Count Technique," *Public Opinion Quarterly*, 74(1): 37-67.

[9] Bernstein, Robert A., Anita Chandha and Robert Montjoy 2003. "Cross-State Bias in Voting and Registration Overreporting in the Current Population Surveys," *State Politics & Policy Quarterly*, 3(4): 367-386.

[10] This is may be conservative given that Dr. Ansolabehere has estimated that 64% of non-registered survey respondents report that they are registered to vote; see: Ansolabehere, Stephen and Eitan Hersh 2012. "Validation: What Big Data Reveal About Survey Misreporting and the Real Electorate," *Political Analysis*, 20: 437-459.

data as "messy" and notes that any attempts by researchers to remove deadwood are necessarily imperfect and incomplete. [11]

27. Elsewhere, Dr. Ansolabehere has identified anomalies in Texas voter registration data that may suggest a large number of errors and deadwood in the TEAM database.[12] These include an implausible number of persons with birthdates on November 11th and an unusually large discrepancy between the number of voter registration records in Texas obtained by Catalist in 2010 and the official state count of registered voters at that time. These findings by Dr. Ansolabehere raise concern about the quality of voter registration data in Texas, as well as the quality of data collection and matching by Catalist. However, in his same report, Ansolabehere advises caution in interpreting these numbers and notes that he is "still working" on understanding the state-specific issues with list quality.

28. The presence of invalid records in state voter registration rolls means that the database matching method will produce non-matches for reasons unrelated to whether an individual possesses requisite ID under SB 14.

29. Not only is the quality of state voter registration data well understood to be problematic, but other state and federal databases are also not infallible. For example, one of the experts for the plaintiffs, Mr. Wood, asserts that voter registration record in Texas "often" do not match DPS records.[13] This only serves to increase the expected number of non-matches that will occur for reasons unrelated to whether an individual lacks requisite ID under SB 14.

30. Given the multiple sources of non-matches, it is a reasonable *a priori* expectation that the matching methods employed by experts for the plaintiffs will exaggerate the number and percentage of otherwise eligible voters who lack requisite voter ID. Also, given the studies cited above, the expected number of non-matches for reasons unrelated to possession of voter ID is likely to be quite large. As a first pass estimate, the percentage of such non-matches relative to the number of records in the TEAM data is surely greater than 0% and possibly as high as 24%, or even higher. Failure to account sufficiently for these various sources of non-matches will result in an over-estimate of the number of registered voters who lack voter ID.

31. In his expert report for the plaintiffs, Dr. Herron states there are 13,564,410 registered voters in Texas.[14] Dr. Herron is clear to note that by "registered voters in Texas, or lists thereof" he is referring to "registered voters in the state as of January 15, 2014."[15] However, given the discussion above it is obvious that this figure likely overstates the actual eligible and registered voters by some non-trivial amount.

[11] Ansolabehere, Stephen and Eitan Hersh 2012. "Validation: What Big Data Reveal About Survey Misreporting and the Real Electorate," *Political Analysis*, 20: 437-459.
[12] Ansolabehere, Stephen and Eitan Hersh 2012. "The Quality of Voter Registration Records: A State-by-State Analysis," *CalTech/MIT Voting Technology Project Report* (July 14, 2010).
[13] Report of Randall Buck Wood, p. 6.
[14] Report of Michael Herron, p.6.
[15] Herron, p. 8.

32. Dr. Herron also finds that "conservatively speaking" 1,232,240 registered voters (or 9.08%) "appear to lack access to valid forms of identification per current Texas law."[16] Further, Dr. Herron reports that if the "45,514 registrants who appear to be deceased are ignored, then approximately 9.11% of the registrant pool in Texas lacks identification."[17] This last statement implies that none of the "no match" cases were among those who "appear to be deceased."[18] This immediately raises the concern that some number of the "no match" records in Dr. Herron's analysis are also deceased voters that either can't be determined to be deceased or for some reason haven't been checked to see if they "appear to be deceased." Consequently, this adjustment by Dr. Herron is expected to be incomplete and likely further exaggerates his estimate of the percentage of voters who lack ID.

33. Moreover, given the preceding discussion on the extent of errors and deadwood in state voter registration rolls, Herron's matching methodology is expected to result in a large number of non-matches for reasons that have nothing to do with a voter lacking ID. Nevertheless, Herron asserts that non-matches represent registered voters in Texas who lack requisite voter identification under SB 14. In my judgment this claim is highly exaggerated and misleading.

34. Elsewhere, Dr. Herron exhibits care to emphasize that not all matches should be considered persons who actually possess voter identification. For example, "if a hypothetical registered Texas voter in the TEAM database can be associated with a record in, say, the Texas driver's license database, then, assuming this association is valid, it can be said that the registered voter was issued a valid Texas driver's license that, as of a given date, was still valid."[19] It is unclear why a similar degree of care is not taken in describing non-matches.

35. While it is clear that Dr. Herron has exaggerated the number of real persons who are legally registered voters and lack voter identification, the degree of this exaggeration is not immediately apparent. To get a sense of just how misleading Dr. Herron's reported findings are, it is useful to compare his analysis to that in the expert report of Dr. Ansolabehere.

36. Dr. Stephen Ansolabehere likewise finds, upon initial examination, that about 1.2 million registered voters in Texas (or 9.1%) do not possess acceptable SB 14 photo identification.[20] However, Dr. Ansolabehere also makes a greater effort to remove various non-matches that do not represent real and legally registered voters who lack necessary identification under SB 14. The end result of this process is an estimate of 664,004 remaining non-matches.[21] Taking this latter figure from Ansolabehere's analysis at face value implies that Herron has over-estimated the number of non-matches that represent actual registered voters lacking necessary ID by about 85%.[22]

[16] Herron, p.6.
[17] Herron, p. 6.
[18] Note that 9.11% = 100%*[1,232,240/(13,564,410 – 45,514)].
[19] Herron, p.10.
[20] Ansolabehere, p. 8.
[21] Ansolabehere, p. 41-42 and Table VII.1.
[22] [(1,232,240 – 664,004)/664,004]*100%

37. Moreover, in reviewing Dr. Ansolabehere's report (below), I will show that the estimate of 664,004 registered voters lacking requisite ID is also likely to be over-stated. Consequently, while it is by no means clear exactly how exaggerated Dr. Herron's findings are, it is clear that they are highly exaggerated. Further, Dr. Herron conducts his analysis in a manner that is expected to produce a dramatic over-estimate of the number of registered voters who lack sufficient ID under SB 14.

38. Dr. Ansolabehere states that the TEAM database contains 13,564,416 records as of January 15, 2014. This total is nearly identical to that reported by Dr. Herron in his report (13,564,410 records). Starting from this base, Dr. Ansolabehere finds that "approximately 1.2 million voters in Texas" do not possess "acceptable SB 14 photo identification, representing 9.1 percent of registered voters"; and that "approximately 1.1 million voters in Texas neither possess acceptable SB 14 photo ID nor qualify under SB 14 for a disability-based exemption from showing ID at the polls."[23]

39. These estimates are very similar to those presented by Dr. Herron, but this similarity is largely attributable to the fact that Ansolabehere has chosen to report as findings his initial estimates that likewise do not account for other reasons for non-matches. Ansolabehere even implicitly admits the problem in his findings when he states: "The matching algorithm produces a NO MATCH list, which consists of all records on TEAM for which no matching record could be found in any identification database and which are not recorded in TEAM as having received a disability exemption. *Each record on this list is treated as an individual registered voter who lacks acceptable SB 14 photo ID* (emphasis added)."[24]

40. Given that Dr. Ansolabehere has contributed to the scholarly literature that documents the extent of deadwood and invalid entries in state voter registration rolls (cited above), it is particularly irresponsible and misleading for Ansolabehere to report as a finding that over one million voters in Texas do not possess acceptable ID under SB 14.

41. Ansolabehere's failure to be forthcoming about the "messy" nature of state registration rolls is all the more disconcerting given that buried in his own analysis are results that betray the exaggerated nature of his finding regarding the number of voters who lack requisite ID. In a subsequent section of his report that is included as a validation exercise regarding the estimated percentage point disparities in rates of possession of ID by race and ethnicity, Ansolabehere uses Catalist data to cross-check the validity of records in the TEAM database.[25] In doing so, Ansolabehere removes records that Catalist indicates are deceased, moved, etc. Ansolabehere also removes records of voters that are expected to be exempt from the ID requirements in SB 14. In other words, Ansolabehere is removing some of the non-matches that are expected to arise for reasons other than a voter who lacks necessary voter ID. The end result of this exercise yields a total of 664,004 non-matches that may represent persons who lack voter ID, or about 4.9% of the original 13,564,416 records in the TEAM database.[26]

[23] Ansolabehere, p.2.
[24] Ansolabehere, p. 7.
[25] Ansolabehere, p. 37-44.
[26] Ansolabehere, p. 41-42 and Table VII.1.

42. For the moment, assume that the "hidden finding" of 664,004 registered voters who lack ID as reported by Ansolabehere in his subsequent validation exercise is not itself an over-estimate. This implies that Ansolabehere's own research contained in his expert report indicates that his finding of 1.1 million voters who lack requisite ID is overstated by about 65%.[27] This is a rather large discrepancy that would seem to merit some explanation or caveat.

43. Further, Ansolabehere reports that the 664,004 registered voters who lack ID represent 6.2% of the relevant total. While 6.2% is already quite a bit lower than the 9% that is emphasized elsewhere in his report, the 6.2% figure is calculated using a denominator that excludes voters that are registered but not required to present SB 14 ID (e.g., those eligible for disability exemptions). In other words, comparing these two percentages is an exercise in comparing apples and oranges. Consequently, the hidden finding of 664,004 is an even lower percentage when applied to a more relevant base population.

44. Even so, this "hidden finding" of 664,004 voters who lack requisite voter ID is likely an over-estimate, as well. Dr. Ansolabehere has previously employed Catalist data to validate state voter registration and voting history records. In one recent study, Ansolabehere speculates that Catalist may not be able to identify a large number of "actual dead voters" on state registration rolls.[28] In another study, Ansolabehere notes that while Catalist may be able to identify some instances of deadwood by comparing state registration records to the National Change of Address (NCOA) registry, this procedure still misses people who do not register their move with the Post Office.[29] Consequently, Ansolabehere is well aware that the filters he employs to screen out deadwood from the TEAM database in his validation exercise are expected to miss some cases of invalid registrations. This in turn implies that the 664,004 non-matches described above will likewise over-state the number of registered voters that do not possess the requisite ID under SB 14.

45. While it is apparent that even the "hidden finding" in Ansolabehere's report is an overestimate of the number of registered voters who lack requisite lack voter identification, the extent of the upward bias is not immediately apparent. However, my review of the evidence from the survey conducted by Dr. Matt Barreto and Dr. Gabriel Sanchez suggests that even Ansolabehere's "hidden finding" dramatically overstates the number of registered voters who lack ID, perhaps by a factor of three or more (see below).

46. Consequently, while it is unclear exactly how exaggerated Dr. Ansolabehere's findings are, it is clear that they are highly exaggerated. As with Herron, Dr. Ansolabehere conducts his analysis in a manner that is expected to produce a dramatic over-estimate of the number of real legally registered persons who lack sufficient ID under SB 14. Finally, Dr. Ansolabehere chooses to highlight his initial estimate of about 1.2 million (9%) registered voters who lack requisite ID, even though his own analysis reveals this claim to be highly exaggerated and misleading.

[27] [(1,100,000 – 664,004)/664,004] * 100%

[28] Ansolabehere, Stephen and Eitan Hersh 2012. "The Quality of Voter Registration Records: A State-by-State Analysis," CalTech/MIT Voting Technology Project Report (July 14, 2010).

[29] Ansolabehere, Stephen and Eitan Hersh 2012. "Validation: What Big Data Reveal about Survey Misreporting and the Real Electorate," *Political Analysis*, 20: 437-459.

47. Dr. Bazelon bases his findings regarding the burden of SB 14 on the number of "Affected Registered Voters." Bazelon defines "Affected Registered Voters" as "registered voters whose only option is to obtain a Required ID in order to vote."[30] In turn, "Required ID" is defined by Bazelon to be those forms of ID required by SB 14.[31]

48. Bazelon determines the number of Affected Registered Voters by employing a no match list provided by the Department of Justice.[32] Bazelon reports that the TEAM database contains 13,564,410 voters; this figure is identical that cited by Herron. Bazelon then omits from his analysis those records that cannot be matched to Census blocks or are located in a Census block with a prison; this yields 13,403,109 "Registered Texas Voters."[33] From this starting point, Bazelon reports that there are 1,232,231 non-matches and 1,103,491 non-matches that are not disability exempt.

49. The total number of persons who lack Required ID in Dr. Bazelon's report is very similar to that reported by Dr. Ansolabehere; the difference appears to be attributable to the omission by Bazelon of those records that cannot be matched to Census blocks or are located in blocks with prisons. However, Dr. Bazelon does not acknowledge the well-established "messiness" in voter registration rolls that is expected to bias his findings. Consequently, his findings regarding the number voters without requisite ID under SB 14 are likewise exaggerated and unreliable.

50. Other expert reports submitted by the plaintiffs cite Ansolabehere's misleading and highly exaggerated finding regarding the number of voters who lack requisite ID under SB 14.[34] In at least two instances, the experts are political scientists (Burden and Davidson) who should have the expertise to recognize that Ansolabehere has greatly over-stated the number of voters who lack ID.

51. In his expert report, Dr. Burden cites Ansolabehere's highly exaggerated finding regarding the 1.2 million registered voters in Texas who lack requisite ID.[35] Burden does not provide any caveats, nor does he acknowledge any concerns regarding this biased and misleading over-estimate.

52. Chandler Davidson also uncritically cites Ansolabehere's highly exaggerated finding that "approximately 1.2 million Texas registered voters lack an accepted form of SB 14 ID..."[36] Chandler likewise does not provide and caveats, nor does he express any qualms about this biased and misleading over-estimate.

53. As noted at the start of this section, any estimate involves error and the potential for bias. It is customary in scientific inquiry to acknowledge the existence of such error by providing some range for the estimated value. Contrary to this practice, experts for the plaintiff present the estimated number of voters who lack requisite ID under SB 14 as a specific number (e.g., 1.1 million or 1.2 million) or

[30] Report of Colemon Bazelon, p. 3.
[31] Bazelon, p. 2.
[32] Bazelon, p. 6.
[33] Bazelon, p. 15 (Table 1).
[34] E.g., Report of Jane Henrici, p. 13.
[35] Report of Barry Burden, p.5 and p. 25.
[36] Report of Chandler Davidson, p. 41-42.

percentage of registered voters. This serves to understate the "messiness" inherent in the matching process and is therefore misleading regarding the confidence that should be placed in such estimates. However, far more problematic in my opinion is the failure of these experts to acknowledge the dramatic upward bias in these estimates. This obfuscation renders their findings unreliable and misleading.

Public Opinion Survey Method

54. In their expert report for the plaintiffs, Dr. Matt Barreto and Dr. Gabriel Sanchez present evidence from a survey of self-reported eligible voters in Texas. Based on this survey they find that approximately 1.2 million eligible voters in Texas lack the requisite ID under SB 14. However, this estimate is likely biased upward, since the methods employed by Barreto and Sanchez are likely to overstate the number of otherwise eligible voters who lack requisite ID for several reasons.

55. First, other than a final validation question and an initial screening question that asks if respondents are age 18 or older, "have lived in Texas over 30 days" and are citizens, Barreto and Sanchez do not seek to confirm the voting eligibility of respondents.[37] A more thorough check is possible, given that the survey includes questions about year of birth, residence and citizenship. For example, my examination of their data reveals that 210 respondents do not report their year of birth; 87 respondents do not report how long they have resided in Texas; and 66 respondents report that they were born in another country but do not report when they became citizens.[38] All of these cases represent failures to affirm eligibility, which at least renders the eligibility of these respondents suspect.

56. Respondents are also asked about whether "official voter records at the Secretary of State's office indicate that you are currently registered to vote here in Texas."[39] Strictly speaking, respondents can't really know the answer to this question. Further, a respondent that is not legally registered at their current residence (due to a recent move) could well answer affirmatively to this question. Also, it is well known that survey respondents over-report registration.[40]

57. In general, survey responses are subject to "motivated reasoning" by respondents; that is, respondents may systematically misreport on surveys in a manner that fits their preconceptions or preferences (ideological and partisan beliefs are particularly important drivers of motivated reasoning in surveys).[41] In the present context this implies that people who oppose voter ID laws very strongly may

[37] Barreto and Sanchez, "Texas Survey Instrument" p. 1.
[38] Several respondents also reported that they have lived in Texas longer than their self-reported year of birth indicates is possible. I ignore this discrepancy except to note that it underscores the inherent problems of reporting errors in surveys, as well as providing an indication that Barreto and Sanchez conduct their analysis of these survey data with excessive credulity.
[39] Barreto and Sanchez, "Texas Survey Instrument," p. 1.
[40] Ansolabehere, Stephen and Eitan Hersh 2012. "Validation: What Big Data Reveal About Survey Misreporting and the Real Electorate," *Political Analysis*, 20: 437-459.
[41] E.g., Taber, Charles and Milton Lodge. 2006. "Motivated Skepticism in the Evaluation of Political Beliefs," *American Journal of Political Science*, 50(3): 755-769; and Bolsen, Toby, James N. Druckman, and Fay Lomax Cook. 2014. "The Influence of Partisan Motivated Reasoning on Public Opinion," *Political Behavior*, 36: 235-262. Also see

be motivated to report that they do not possess ID, even when they do. Likewise, people that support ID laws may misreport in the other direction. But since the vast majority of eligible voters do possess ID, the net effect of this misreporting is likely to result in an overestimate of the number of persons without the requisite ID under SB 14.

58. Yet another source of upward bias in Barreto and Sanchez's estimates of the number of respondents without requisite ID under SB 14 comes from the fact that they subject respondents to a lengthy and repetitive set of questions regarding multiple forms of ID. In some instances, respondents are even asked to produce an ID and examine it.[42] These are probably unexpected questions for "a short public opinion survey about important issues in Texas" and it is easy to imagine that not all respondents choose to cooperate by answering completely and honestly.[43] Given that an easy way to cut short this sequence of questions is to report not having a particular form of ID, Barreto and Sanchez may well overstate the number of persons who lack requisite voter ID.

59. Further, after subjecting respondents to a lengthy and repetitive set of questions about identification documents, Barreto and Sanchez then ask respondents whether there is any difference in the name on their ID and "your name as it *might* appear on the official voter registration card (emphasis added)."[44] This is a hypothetical question that presumably most respondents will have no means to answer definitively. Consequently, the responses to this question are highly suspect and may be particularly susceptible to motivated reasoning.

60. At first glance, the reported finding by Barreto and Sanchez that 1.2 million eligible voters lack requisite ID in Texas appears to be consistent with the findings reported by Ansolabehere, Herron and Bazelon. However, Barreto and Sanchez employ a different divisor than these other experts. Barreto and Sanchez use the results of their survey to extrapolate the number of citizens of voting age (CVAP) that lack voter ID in Texas, not the number of registered voters.[45]

61. Barreto and Sanchez use an estimate of CVAP from the 2008-2012 American Community Survey. In doing so, they likely understate the 2014 CVAP in Texas.[46] However, it is also well known that CVAP overstates the voting eligible population (VEP).[47] It is unclear how these two different sources of error affect their estimate of the number of eligible voters without requisite voter ID.

Beaulieu, Emily. 2014. "From Voter ID to Party ID: How Political Parties Affect Perceptions of Election Fraud in the U.S." *Electoral Studies*, 35: 24-32.

[42] Barreto and Sanchez, "Texas Survey Instrument," p. 4-5.

[43] Barreto and Sanchez do not report how many respondents fail to complete the survey after starting, but survey length, complexity and subject interest are all factors that are generally understood to affect the quality of responses in surveys (e.g., McCarty, Christopher, House, Mark, Harman, Jeffrey, and Scott Richards. 2006. "Effort in Phone Survey Response Rates: The Effects of Vendor and Client-Controlled Factors," *Field Methods*, 18: 172-188).

[44] Barreto and Sanchez, "Texas Survey Instrument," p. 6.

[45] Report of Matt Barreto and Gabriel Sanchez, p. 9.

[46] Barreto and Sanchez, p. 11.

[47] McDonald, Michael P. and Samuel L. Popkin. 2002. "The Myth of the Vanishing Voter." *American Political Science Review*, 95(4): 963–74.

62. Taking the findings in Barreto and Sanchez at face value permits a comparison to the estimates reported by other experts for the plaintiff. For example, Barreto and Sanchez report that 3.8% of registered voters do not possess requisite ID under SB 14.[48] This is much lower than the approximately 9% reported in Ansolabehere and elsewhere. This estimated percentage is even lower than Ansolabehere's "hidden finding" that 6.2% of eligible registered voters lack requisite ID (or 664,004).[49]

63. I have analyzed the survey data provided by Barreto and Sanchez along with their report for the purpose of performing some rudimentary quality checks. This exercise reveals several additional problems with their analysis. It also reveals that Barreto and Sanchez make several questionable assumptions regarding whether a respondent has ID; these assumptions serve to exaggerate both the estimated percentage of respondents without requisite ID and the differences in rates of ID possession by race and ethnicity.[50]

64. Barreto and Sanchez incorrectly identify the number of white Hispanic respondents, as well as how the U.S. Census defines these categories. They state that:

> "Out of the entire sample of 2,344 respondents there were 5 respondents who said they were both Hispanic and white. In this case, we include these as part of the Hispanic group and not as white, consistent with the United States Census."[51]

First, there are actually 7 respondents that self-identify as both white and Hispanic. Second, the U.S. Census does not conflate race and Hispanic origin. For example, both the Current Population Survey and the American Community Survey ask separate questions on race and Hispanic origin. In contrast, Barreto and Sanchez as a single question that includes both race and Hispanic ethnicity (but permits multiple responses). As a result, it is unclear exactly how Barreto and Sanchez code the race and ethnicity of their respondents.

65. It is also unclear how Barreto and Sanchez classify the race and ethnicity of the 34 respondents who do not report a race or ethnicity. Further, it is unclear how they classify the 24 respondents who specify an "other" response, some of which are nationalities, or answers such as "Texan" or "hHuston" (sic).

66. Based on my understanding of their statistical analysis, I also find anomalies in the manner in which Barreto and Sanchez determine whether a respondent has the requisite ID under SB 14. In each case, they resolve ambiguity in a manner that leads to counting more respondents as not having requisite ID.

[48] Barreto and Sanchez, Appendix p. 1 (Table 1).
[49] Ansolabehere, Table VII.1.
[50] Barreto and Sanchez do not provide an explicit description of the final variables employed in their analysis, nor do they provide the code to create these variables. It is also not apparent how to reverse engineer their coding from the reported results. For example the variable labeled "id_type" in the raw data contains several inconsistencies with other indicators of whether a person has requisite ID. Consequently, I base my understanding of how Barreto and Sanchez code variables on their report, survey instrument and the raw survey data, but I cannot be certain of how they coded variables.
[51] Barreto and Sanchez, footnote 7, p. 8.

67. First, Barreto and Sanchez include individuals with suspended or revoked driver's licenses among the group that lacks requisite ID.[52] This is incorrect, since a suspended license still meets the SB 14 definition of a "Texas driver license issued by the Texas Department of Public Safety (DPS)."[53] It is my understanding that in Texas some drivers may have their licenses confiscated at the time of suspension, but that other drivers instead receive a Notice of Suspension by mail from the Texas Department of Public Safety.[54] Consequently, it is quite possible for an individual with a suspended driver's license to still be in possession of that license and to use it for the purposes of voter ID under SB 14. My understanding regarding the validity of a suspended license for voting identification purposes is also corroborated by one of the experts for the plaintiff, Dr. Allan Littman. [55]

68. Second, not all respondents provide a clear "Yes" or "No" answer when asked if they possess a valid and unexpired driver's license; some respond that they do not know or simply refuse to answer. However, Barreto and Sanchez classify persons who do not unambiguously answer such questions as not possessing this form of ID. For example, 3 respondents do not report whether they have a driver's license. Another 11 do not answer whether their license is valid, lost or stolen, revoked or canceled. Another 30 respondents do not answer whether their license is expired or updated in the last 6 years. Barreto and Sanchez appear to treat these cases as respondents without a valid and unexpired form of ID. This is a particularly unwarranted assumption that serves to exaggerate their count of the number of respondents without requisite ID.

69. Third, not all respondents are willing and able to examine their driver's license and report whether it is expired or not; in these cases, Barreto and Sanchez ask: "Well, when was the last time you went and had your driver's license updated? Was it in the last 6 years, since March 2008, or was it sometime BEFORE that?"[56] This question ignores the fact that licenses can be renewed online, by phone or through the mail.[57] Further, it strains credulity to expect that a respondent can answer definitively about events up to 6 years in the past. Further, respondents are not asked whether they have "renewed" their license in the last 6 years, or whether they first obtained their license in the last 6 years. My understanding is that a person can "update" the name, address or gender on their license with or without renewing the license at the same time.[58] Consequently, this survey question really doesn't identify respondents that have not renewed their license in the last six years, even if it were reasonable to expect accurate recall of such distant events. There are another 6 respondents that are classified by Barreto and Sanchez as not possessing an unexpired driver's license because of this poorly designed question.

[52] A total of 15 respondents report that they have a suspended driver's license.
[53] http://votetexas.gov/register-to-vote/need-id/; last viewed July 25, 2014.
[54] http://www.dps.texas.gov/internetforms/Forms/DIC-25.pdf; last viewed July, 25, 2014.
[55] Report by Allan Lichtman, p. 34.
[56] Barreto and Sanchez, "Texas Survey Instrument," p. 4.
[57] http://www.txdps.state.tx.us/DriverLicense/renewal.htm; last viewed July 25, 2014.
[58] http://www.txdps.state.tx.us/driverlicense/changes.htm; last viewed July 25, 2014.

70. Fourth, not all respondents provide a clear "Yes" or "No" answer when asked if they possess some other valid and unexpired form of ID. For example, 5 respondents do not answer whether they have an unexpired non-driver photo ID card form the DPS; 22 do not answer definitively whether their non-driver ID is expired or not; 10 do not answer whether they have a Texas Election Identification Certificate; 5 do not answer whether they have a passport; 21 do not answer definitively whether their passport is expired or not; 4 do not answer whether they have a military ID card; 5 do not answer whether their military ID is expired or not; 8 do not answer whether they have a concealed handgun license or a citizenship certificate; and 2 do not answer whether their concealed handgun license is expired or not. Once again, it is an unwarranted assumption to treat any of these respondents as if they do not have voter ID; doing so serves to exaggerate the number of respondents who do not have ID.

71. Barreto and Sanchez also ask respondents whether the name printed on their ID is an exact match to the name as "it would appear on your official voter registration card." This again requires respondents to provide a definitive answer when they are most likely not simultaneously examining both their photo ID and official voter registration card. Consequently, answers to this question are particularly susceptible to misreporting.

72. Having identified several questionable assumptions regarding which respondents possess an unexpired ID, I next check the sensitivity of some of the findings in Tables 1-2 of Appendix A of the report by Barreto and Sanchez. Given the limited time available to me, I have not attempted to reexamine all of the findings in Barreto and Sanchez, nor is the intent of this analysis to correct all of the problems in their analysis. However, the examples chosen suffice to demonstrate the existence of an upward bias in their analysis. For this sensitivity analysis, I select four specifications that are intended as reasonable alternatives to some of the unwarranted assumptions made by Barreto and Sanchez.

73. In Replication 1, I define respondents without ID in the same manner as Barreto and Sanchez (as best I can tell from their report), except that I omit those respondents who give ambiguous responses to whether they have an unexpired ID. Specifically, I omit responses that give answers of "don't know," "maybe/not unsure/can't remember" or outright refuse to provide an answer; I also omit those respondents that report having a suspended driver's license, since it is unknown whether the respondent still has the license in question.

74. Replication 2 is identical to Replication 1, except that I follow Dr. Ansolabehere and take into account respondents who are eligible for a disability exemption or are eligible for vote by mail (age 65 and older). These respondents do not lack requisite ID, so are coded accordingly.

75. Replication 3 is identical to Replication 2, except that I drop any respondents that provide responses that render their eligibility to vote ambiguous, or who refuse to indicate their race and ethnicity. This includes any respondents that refuse to report their age, disability status, state of residence, or year of citizenship (where applicable). I also omit those respondents that report both not possessing requisite ID and report voting after the implementation of SB 14.

76. Replication 4 is similar to Replication 2, except that rather than dropping respondents that give ambiguous responses regarding ID, I code these as possessing requisite ID.

77. The results of applying these alternative definitions for which respondents do or do not lack an unexpired SB 14 ID are reported in Table 1. I reproduce the original results from Barreto and Sanchez for comparison.

Table 1: Percent of Survey Respondents who DO NOT Possess a Valid and Unexpired License

	White	Black	Hispanic	Total
Barreto and Sanchez, Table 1 in Appendix A	4.7%	8.4%	11.4%	7.2%
Replication 1	3.5 (1.1)	5.5 (1.6)	4.3 (1.2)	4.0 (0.8)
Replication 2	3.2 (1.0)	4.5 (1.4)	3.2 (1.1)	3.2 (0.7)
Replication 3	3.4 (1.1)	4.8 (1.6)	2.5 (1.0)	3.1 (0.7)
Replication 4	3.2 (1.0)	4.3 (1.4)	3.0 (1.1)	3.1 (0.7)

Note: Percent of self-reported voting eligible respondents. Weighted means using survey weights; standard errors in parentheses are from a weighted regression.

78. In every case, the alternative specifications indicate lower rates of voters who do not have unexpired ID. This exercise gives a sense of how sensitive Barreto and Sanchez's findings are to alternative and reasonable specifications. For example, compared to Replication 4, Barreto and Sanchez exaggerate the percentage of eligible voters who lack SB 14 ID by 132%.[59]

79. Of course, the estimated percentage of eligible voters generated by these alternative specifications is still subject to several of the upward biases identified above. Consequently, the actual percentage of eligible voters who lack an unexpired SB 14 ID is likely even smaller than any of the figures reported in Table 1.

80. In Table 2, I report the results of applying these same alternative definitions to the subsample of respondents who affirm that they are registered to vote. Once again, in every case the percentage of voters who do not have unexpired ID is lower than reported by Barreto and Sanchez.

81. Taking the results in Table 2 at face value provides a check on the findings reported by other experts. For example, in contrast to the finding that about 9% of registered voters lack requisite ID reported by Ansolabehere, this exercise suggests that fewer than 2% of all registered voters lack SB 14 ID. In other words, re-analyzing the data in Barreto and Sanchez and taking the results at face value suggests that the findings reported by Ansolabehere are exaggerated by about 350%.[60] Of course, for the same reasons discussed above, the results of this sensitivity analysis are expected to overstate the actual number of registered voters who lack unexpired SB 14 ID.

[59] [100% * (7.2 – 3.1)/3.1].
[60] [100% * (9 – 2)/2].

Table 2: Percent of Registered Survey Respondents who DO NOT Have a Valid and Unexpired License

	White	Black	Hispanic	Total
Barreto and Sanchez, Table 1 in Appendix A	2.1%	4.9%	6.8%	3.8%
Replication 1	1.4 (0.7)	2.5 (1.0)	3.1 (1.1)	1.9 (0.5)
Replication 2	1.1 (0.7)	2.2 (1.0)	1.9 (0.9)	1.4 (0.5)
Replication 3	1.0 (0.7)	2.0 (1.1)	2.3 (1.1)	1.5 (0.5)
Replication 4	1.1 (0.7)	2.1 (0.9)	1.9 (0.9)	1.4 (0.5)

Note: Percent of self-reported voting eligible and registered respondents. Weighted means using survey weights; standard errors in parentheses are from a weighted regression.

82. I have demonstrated that Barreto and Sanchez make a number of questionable assumptions that serve to bias their estimate of the number of voters in Texas without requisite ID. I also demonstrate that the extent of the upward bias in their estimates is quite large. Further, this demonstration by no means corrects for all of the problems in their analysis. Consequently, the analysis in Barreto and Sanchez is unreliable and their findings regarding the number or percentage of voters without requisite ID are highly exaggerated and misleading.

Summary of Section V: Experts for the plaintiffs assert than more than one million otherwise eligible voters in Texas lack SB 14 ID (or about 9% of registered voters). I demonstrate that these estimates are highly exaggerated. Moreover, I demonstrate that the methods employed by experts for the plaintiffs are expected to generate a large upward bias in the estimated number of eligible voters without ID. Given this, the analyses proffered by experts for the plaintiffs relating to the number or percent of voters who lack SB 14 ID is unreliable and misleading.

VI. Differences in the Rates of Possession of ID

83. The preceding section demonstrates that the challenges associated with credibly estimating eligible voters without SB 14 ID are quite daunting; these challenges are compounded when trying to estimate differences in rates of ID possession by race and ethnicity. The TEAM database does not include racial or ethnic identifiers, so once again experts for the plaintiff attempt to estimate these differences.

84. Any such estimate involves some degree of uncertainty and is subject to bias. In this section, I demonstrate that experts for the plaintiffs do not sufficiently acknowledge the uncertainty and bias in their estimates of differences in rates of possession of voter ID by race and ethnicity, nor do they take sufficient care to reduce the bias in their estimates. Consequently, the estimates of the number or percentage of voters in different racial or ethnic groups in Texas who lack requisite ID proffered by experts for the plaintiff are overstated and unreliable.

85. Experts for the plaintiffs employ three different methods for estimating the difference in rates of ID possession by race and ethnicity. The first attempts to estimate differences in rates of possession of ID by exploiting information on the racial and ethnic composition of geographic areas within Texas. The second attempts to match individual records to external data that does contain information on race and

ethnicity. The third method involves analyzing responses to an opinion survey. I consider each of these methods in turn.

Estimating Differences Using Matching Methods

86. Dr. Bazelon bases his analysis on his own severely exaggerated list of "Affected Registered Voters." Bazelon states: "Essentially, my method only detects a racial disparity based on where Affected Registered Voters live."[61] However, given the serious deficiencies in Bazelon's estimates of the number of registered voters who do not possess requisite voter ID, his analysis of differences in the rates of possession between white, black and Hispanic Texans is unreliable. However, even ignoring this fundamental and fatal flaw, there are other errors in Bazelon's analysis that likely serve to exaggerate his reported difference in the rates of possession of ID across groups.

87. In order to assign a race and ethnicity to these individual observations, Bazelon links each record to a Census Block Group and assigns a probability that each record represents a white, black or Hispanic voter based on the proportion of registered voters of each category that are in that Census Block Group.[62] Bazelon employs registration data from the Current Population Survey.[63] As noted above, the CPS is expected to overstate registration, so this is another source of error and potential bias in Bazelon's analysis.

88. I have already established that Bazelon's estimate of the number of persons without ID is likely highly exaggerated and contains a large number of non-matches for reasons other than the lack of a requisite voter ID. To the extent that these non-matches are correlated with minority race or Hispanic ethnicity, then Bazelon's estimated differences in rates of ID possession will also be biased upward.

89. However, even if the many and severe problems in Bazelon's methods are ignored, his own analysis reveals evidence that is difficult to reconcile with the assertion that SB 14 is intended to be a racially discriminatory electoral strategy. Taking Bazelon's estimates at face value, consider the number of registered non-Hispanic white voters that Bazelon estimates are adversely affected by SB 14. This estimate is larger than the similarly estimated number of either black or Hispanic affected voters (and nearly as large as the sum of those two groups).[64]

90. Further, according to Bazelon, non-Hispanic whites make up about 48% of the "EIC required" voters in Texas.[65] However, according to Barreto and Sanchez, non-Hispanic whites make up only about 45% of the citizen voting-age population in Texas (this is based on 2008-2012 ACS data, so an updated estimate would likely find that today non-Hispanic whites make up an even smaller percentage of CVAP).[66] Consequently, taking Bazelon's analysis at face value suggests that non-Hispanic whites are disproportionately likely to lack SB 14 ID relative to their share of CVAP.

[61] Bazelon, p. 13 and Appendix D.
[62] Bazelon, p. 13.
[63] Bazelon, p. 13.
[64] Bazelon, p. 15 (Table 3).
[65] [100% * (530,636/1,103,491)].
[66] Barreto and Sanchez, p. 11.

91. Bazelon reports the number and percent of "Affected Registered Voters" broken down by white, black and Hispanic voters, but he does not report similar figures for other races. However, it is possible to calculate the percent of "other race" voters in the Affected Registered Voters category. Based on the data presented in Table 1 of Bazelon's report, approximately 6.7% of other minority voters are similarly "Affected."[67] This exercise is provided just to note that other minorities have the lowest rate of being affected by SB 14, according to Bazelon's results.

92. While it is clear that Bazelon's estimates of Affected Registered Voters are hopelessly flawed, it is not immediately clear just how exaggerated his findings are when it comes to the differences in rates of ID possession. In order to get a sense of the magnitude of the bias in Bazelon's results, it is instructive to compare his estimates to those reported by Barreto and Sanchez (as well as my re-examination of their survey data). For example, Bazelon finds that more than 10% of black and Hispanic voters are affected by SB 14; in contrast, Barreto and Sanchez report that fewer than 7% are similarly affected, while my re-analysis suggests an even lower percentage. However, as emphasized throughout my report, these figures are all expected to be overstated.

93. Dr. Herron takes a somewhat different approach to estimating racial and ethnic differences in rates of ID possession. However, as with Bazelon, his analysis is based upon a highly exaggerated estimate of the number of voters who lack SB 14. Consequently, Herron's analysis is likewise fundamentally and fatally flawed.

94. Even ignoring the deficiencies in the baseline estimates employed by Herron, there remain sources of likely bias that exaggerate the extent to which blacks and Hispanics lack ID relative to non-Hispanic whites. For example, to the extent that non-matches for reasons other than lack of ID are more frequent for records that are matched to disproportionately black or Hispanic geographic locations, then Herron's method is expected to overstate the difference in rates of ID possession in a manner that makes it appear as though blacks and Hispanics are less likely to possess requisite voter ID.

95. Nevertheless, it is instructive to consider the implications of taking Dr. Herron's estimates at face value, if only to get a sense of the magnitude of the bias in his findings. Herron reports several different estimates, so for ease of exposition I will focus his "conformable block group analysis" described in Table 5 and the surrounding text in his report.[68] Herron finds that the percentage of voters lacking SB 14 ID is about 5.6% for whites, 18.5% for blacks, and 15.4% for Hispanics. These percentages are very different than those presented by Bazelon and Barreto and Sanchez (as well as my re-analysis of the survey data in Barreto and Sanchez). Herron finds much higher rates of black and Hispanic voters who lack requisite ID, as well as the largest percentage differences for those groups compared to white voters. This underscores just how exaggerated Herron's findings are, given that the percentages reported by these other experts have already been demonstrated to dramatically overstate these differences across racial and ethnic categories. Consequently, Herron's findings regarding the difference in rates of ID possession by race and ethnicity are unreliable and highly misleading.

[67] [(1,103,491 - 350,224 – 530,636 – 185,095)/(13,403,109 – 3,400,136 – 7,714,425 – 1,730,293)]*100%
[68] Herron, p. 34-35.

96. Dr. Ansolabehere employs two different methods for estimating differences in the rates of lack of SB 14 ID possession by race and Hispanic ethnicity. I discuss each in turn.

97. The first method is very similar to that employed by Herron. Ansolabehere starts by assigning records in the TEAM database to Census Block Groups and estimates the differences in rates of possession of SB 14 ID via ecological regression analysis. Like Herron, Ansolabehere bases his analysis on a wildly exaggerated estimate of the number of persons who lack SB 14 ID. Consequently, Ansolabehere's analysis is also fundamentally and fatally flawed.

98. Even if the serious shortcomings in Ansolabehere's baseline estimates are ignored, his findings are likely to be biased in the same direction and for the same reason as Dr. Herron's. That is, to the extent that minority voters are more likely to be in the set of non-matches that occur for reasons other than the lack of SB 14 ID, the estimated differences in lack of ID possession by race will be exaggerated accordingly. For example, minorities may be more likely to move without submitting a formal change of address form to the post office.

99. Despite the flaws Ansolabehere's ecological regression analysis, it is instructive to consider his findings at face value, if only to get a sense of the magnitude of the bias in his estimates of the percentages of voters who lack SB 14 ID by race and ethnicity. In Table VI.1 of his report, Ansolabehere finds that about 5.1% of non-Hispanic whites, 14.7% of blacks and 10.0% of Hispanics are estimated to lack SB 14 ID. These percentages imply a much larger difference between black and non-Hispanic white voters than reported by Bazelon (9.6% vs. 3.8%), but a much smaller difference than reported by Herron (9.6% vs. 12.9%). Similarly, the estimated difference between Hispanic and non-Hispanic white voters vary according to Ansolabehere, Bazelon and Herron (4.9% vs. 3.4% vs. 9.8%, respectively). The wide range of reported differences does not inspire confidence, especially since these experts are all employing similar methods.

100. Dr. Ansolabehere also attempts to estimate the difference in rates of possession of SB 14 ID by matching TEAM records to Catalist data. The findings reported in Table V1.2 of Ansolabehere's report are similar to those derived from his ecological regression analysis. Dr. Ansolabehere finds that about 7.4% of non-Hispanic whites, 15.1% of blacks and 11.3% of Hispanics lack SB 14 ID.

101. Taking Ansolabehere's analysis at face value again reveals results that are difficult to reconcile with the claim that SB 14 is intended to be a racially discriminatory electoral strategy. Ansolabehere estimates that more non-Hispanic white registered voters lack SB 14 ID than black and Hispanic registered voters combined.[69] Further, Ansolabehere estimates that non-Hispanic whites comprise 49.9% of registered voters without SB 14 ID.[70] As noted above, this is greater than the non-Hispanic white share of the citizen voting-age population in Texas (according to Barreto and Sanchez). Further, Ansolabehere's findings also indicate that other minorities (non-black, non-Hispanic) are the least likely group to lack SB 14 ID.

[69] Ansolabehere, Table VI.2; [614,522 > (258,648 + 343,097)].
[70] Ansolabehere, Table Vi.2; [100% * (614,522/1,232,246)].

102. However, once Ansolabehere makes an effort to remove non-matches that are expected to occur for reasons other than a voter who lacks requisite ID, the estimated number and percentages of voters without ID in each group are much lower. However, just comparing the percentages reported in Table VI.2 to Table VII.1 will understate this drop, since Ansolabehere changes the denominator in calculating those percentages. It is more instructive to observe the percentage change in the number of voters estimated to be without requisite ID with and without this correction. For example, the estimated number of non-Hispanic whites without requisite ID is about 94% higher in column 1 of Table VI.2 than in column 4 of Table VII.1.[71] Similar calculations reveal that the findings reported in Table VI.2 for black, Hispanic and other voters are about 81%, 76% and 84% higher (respectively) than reported in Table VII.1. These are very large differences, yet Ansolabehere chooses to emphasize the most exaggerated findings that even his own sensitivity analysis reveals to be unreliable. If ever a sensitivity analysis were to reveal some problem with findings, this would be such a case.

103. The hidden findings in Table VII.1 of Ansolabehere's report demonstrate just how exaggerated are his other reported findings (and by extension the findings in Bazelon and Herron). However, even the estimates in Table VII.1 are likely to dramatically overstate the number of voters who lack requisite ID and the percentage differences across racial and ethnic groups.

104. It has already been demonstrated in the preceding section of this report that Ansolabehere's hidden finding is likely to overstate the estimated number of persons without ID. However, the estimated percentage differences across groups are even more problematic. This is in part because the number of remaining non-matches for reasons other than a voter that lacks requisite ID may be correlated with minority race or Hispanic ethnicity. But more importantly, Ansolabehere fails to account for the quality of race and ethnicity estimates in the Catalist data.

105. The expert report submitted by Dr. Ghitza, he notes Catalist continuously updates its database, including voting records, "as frequently as every week."[72] Ghitza also states that the probability that a record is coded correctly as corresponding to a black or Hispanic individual is much lower than for non-Hispanic whites.[73] Further, to the extent that these probabilities are even lower for individuals with low socioeconomic status, it follows that the race and ethnicity of individuals that appear to lack ID are particularly uncertain.

106. The error known to exist in Catalist estimates of race and ethnicity is not addressed by Ansolabehere. Further, the fact that these errors are systematic in that they are expected to be correlated with both lack of ID and minority status compounds this problem. By ignoring the quality of the race and Hispanic ethnicity coding from Catalist, Ansolabehere likely understates the standard errors in his estimated differences in rates of ID possession (and therefore overstates the statistical significance of these differences).

[71] [100% * (614,522 – 317,057)/317,057)].
[72] Report by Yair Ghitza, paragraph 6.
[73] Report submitted by Yair Ghitza, paragraph 15.

107. One possible method for addressing this problem with race and ethnicity identifiers in the Catalist data would be to weight observations by the probability that race or ethnicity is estimated correctly. Another possibility would be to estimate an instrumental variables regression, where the instruments are the probability that a record has been coded correctly. In either case, it is my expectation that these adjustments would have the potential to dramatically reduce the estimated differences in rates of ID possession and\or the statistical significance of the estimated difference.

108. In his report, Ansolabehere emphasize what are expected to be his most exaggerated estimates of both the number of voters without ID in by race and ethnicity, as well as the differences in these rates. Further, his estimated differences using Catalist data fail to account for the particularly low quality of race and ethnicity estimates for minority voters. Consequently, Ansolabehere's analysis of the differences in rates of ID possession by race and ethnicity is unreliable and misleading.

Estimating Differences Using Survey Responses

109. Dr. Barreto and Dr. Sanchez analyze responses to their survey in order to estimate the differences rates of ID possession by race and ethnicity. As demonstrated in the preceding section, Barreto and Sanchez make a number of unwarranted assumptions that serve to greatly exaggerate the estimated number of respondents without voter ID.

110. My re-analysis of the survey data reveals that reasonable alternative specifications generate estimates of the number of voters that are much smaller (see Tables 1 and 2 above). Moreover, the estimated percentages of registered voters without ID are dramatically lower than those reported by experts for the plaintiff. This corroborates my repeated concern that experts for the plaintiff employ methods that are expected to exaggerate the number of voters without ID, as well as the differences in rates of possession of ID by race and ethnicity.

111. Barreto and Sanchez test for differences in rates of ID possession by racial or ethnic group using a logistic regression. I use this same test to check whether the observed differences in Replications 1-4 in Tables 1 and 2 are statistically significant (i.e., $p<.10$). In every case, the observed differences are not statistically significant. In other words, the null hypothesis that black and\or Hispanic voters are no more likely to lack requisite voter ID than non-Hispanic white voters cannot be rejected.

112. My re-examination of the survey data in Barreto and Sanchez confirms that experts for the plaintiff have employed methods that greatly exaggerate the estimated differences in rates of ID possession across racial and ethnic groups. Using the same data and statistical tests as Barreto and Sanchez, even a minor correction to one of their many unwarranted assumptions reveals that there are no statistically significant differences in rates of ID possession among non-Hispanic white, black or Hispanic respondents.

Summary of Section VI: Experts for the plaintiffs assert that black and Hispanic voters in Texas are significantly more likely to be without requisite voter ID under SB 14. These estimated differences vary considerably across experts; however, I demonstrate that all of these estimates are highly exaggerated. Moreover, I demonstrate that the methods employed by experts for the plaintiffs are expected to yield exaggerated estimates of the differences in rates of ID possession by race and

ethnicity. In contrast, my re-examination of the survey data reported by Barreto and Sanchez reveals no statistically significant difference in rates of ID possession by race or ethnicity. Consequently, the analyses proffered by experts for the plaintiffs relating to the disproportionate number of black and Hispanic voters who lack requisite ID under SB 14 ID are unreliable and misleading.

VII. Costs of Obtaining ID

113. Experts for the plaintiff argue that there are costs of obtaining voter ID that are more burdensome for black and Hispanic voters. However, there are fundamental theoretical and empirical problems with these claims.

114. Several experts for the plaintiff argue that the costs of obtaining ID, even a free Election Identification Certificate (EIC), are substantial and particularly burdensome for minority voters in Texas.[74] However, these claims ignore both the ability of individuals to economize on travel time and any concomitant benefits from replacing other missing documents, such as a birth certificate. Consequently, the estimated costs of obtaining voter ID are exaggerated.

115. One cost of obtaining cost an EIC is the opportunity cost of travel time. Both Dr. Chatman and Dr. Webster analyze the travel time in minutes for different types of voters. However, Dr. Bazelon correctly notes that the opportunity costs of time vary by race and ethnicity, so this must be taken into consideration.[75]

116. Dr. Bazelon estimates that the travel cost for obtaining an EIC are on average about $42.[76] Assuming for the moment that this is correct, it interesting to note that Bazelon finds that "Affected" black voters in Texas have lower travel costs than "Affected" white voters (i.e., $28.17 v. $48.79). Consequently, Bazelon's own estimates imply that white voters face costs that are 73% higher than for black voters.[77] Furthermore, Bazelon estimates that the number of Affected Registered white voters is 63% greater than the number of Affected Registered black voters.[78] This contradicts the claims made by several experts that travel costs are higher for minority voters than for white voters in Texas.

117. Bazelon argues that a given black voter is more than twice as likely to require an EIC compared to a non-Hispanic white voter, so that the expected travel costs are higher for black voters in Texas.[79] However, this claim is based on his highly exaggerated estimates of the number of affected voters by race and ethnicity. I have already demonstrated that the estimates made by several experts regarding the difference rates of possession by race and ethnicity are likewise exaggerated. Furthermore, my re-examination of Barreto and Sanchez's survey data reveals no significant difference in the rates of requisite ID possession by race and ethnicity, whether for registered voters or for all eligible voters. Consequently, there is no credible evidence presented by experts for the plaintiffs that expected travel costs are higher for black or Hispanic voters.

[74] E.g. the reports submitted by Daniel Chatman, Jane Henrici, Kevin Jewell, and Gerald Webster.
[75] Bazelon, p. 22.
[76] Bazelon, Table 6 on p. 29.
[77] [100% * (48.79 – 28.17)/28.17)].
[78] Bazelon, Table 1, p. 15; [100% * (530,636 – 185,095)].
[79] Bazelon, p. 29.

118. Bazelon also argues that the principle of diminishing marginal utility implies that a dollar cost imposed on a higher income individual imposes a greater utility burden than a dollar cost imposed on a lower income individual.[80] Bazelon is assuming that imaginary units of utility can be measured and compared across individuals (i.e., that utility is cardinal and comparable). However, this claim does not follow from standard economic theory. In contrast, standard microeconomic theory assumes only ordinal utility; that is individuals can rank items, but these rankings cannot be directly compared across different individuals.[81]

119. Experts for the plaintiff adopt methodologies that exaggerate the travel costs of obtaining IDs. In every instance, these estimates are founded on the assumption that an individual makes a dedicated trip solely for the purpose of obtaining an EIC. This is ridiculous. If a rational person desires to travel to multiple destinations (e.g. the bank, the grocery store and the post office), then they endeavor to minimize travel time by combining activities into as few tips as possible. In general, the costs of travel can be cut dramatically by combining the errand of obtaining an EIC with another activity. In other words, experts for the plaintiff do not estimate the relevant *marginal costs* of obtaining an EIC.

120. Experts for the plaintiffs also fail to consider how the potential costs of obtaining identification may be ameliorated by the actions of neighbors, friends, relatives, co-workers, political groups, and religious or civic organizations. For example, an individual may travel with a friend or relative who also desires to obtain an EIC or has some nearby errand to run. This not only reduces total travel costs, but may yield benefits from time spent together. Alternatively, a civic or political group may organize rides or otherwise facilitate the task of obtaining ID. Individuals desiring assistance in obtaining an EIC are presumably capable of seeking and finding some assistance. Instead, experts for the plaintiff base their estimates of travel costs on the assumption that individuals are isolated, hapless and irrational.

121. Another cost of obtaining an EIC is for the replacement of lost supporting documents, at least for those individuals who do not possess supporting documents needed to obtain an EIC. For example, some individuals may wish to replace a lost birth certificate in order to obtain an EIC. The state of Texas offers a $3 birth certificate for the purpose of obtaining an EIC, or a certified regular birth certificate for $22. The latter document can be used for other purposes over the lifetime of an individual. Consequently, this $22 cost should be apportioned over the expected number of uses that an individual may have in the future. Other than for the $3 birth certificate, it is therefore misleading to assume that the entirety of the cost of any supporting ID is attributable entirely to the cost of obtaining an EIC.

122. An EIC is also valid for 6 years and can be renewed. This means that the costs of obtaining an EIC should be apportioned in some manner over this time. One way of doing so would be divide the costs by the number of elections that a voter will be able to participate in during this 6 year period (perhaps discounting future benefits). Alternatively, the costs of obtaining ID may be apportioned over the

[80] Bazelon, p. 8.

[81] E.g., Goolsbie, Austan, Steve Levitt, and Chad Syverson. 2013. *Microeconomics*. Worth Publishers (New York, NY); Varian, Hal R. 2014. *Intermediate Microeconomics: A Modern Approach*, 9th Edition. W.W. Norton and Company (New York, NY); and Samuelson, Paul.A. 1947. *Foundations of Economic Analysis*. Harvard University Press (Cambridge, MA).

number of races and ballot questions that the voter is now able to participate in over a 6 year period. In either case, the net cost of obtaining ID per-election or per-contest will be dramatically lower than the one-time fixed cost examined by experts for the plaintiff.

123. Even so, obtaining a free EIC will entail some sort of cost. But experts for the plaintiffs do not provide any sense of when such costs are "too high." It cannot simply be the existence of costs that is the problem; all activities involve some opportunity cost, including registering and voting.

124. Finally, experts for the plaintiff do not consider why some individuals lack basic supporting documents, such as a birth certificate. Does the individual bear any responsibility for maintaining a record of citizenship and voter eligibility? What If the same person loses their ID documents multiple times, is the State of Texas responsible?

Summary of Section VII: Experts for the plaintiffs assert that there are substantial costs of obtaining voter identification and that these costs are particularly burdensome for black and Hispanic voters in Texas. However, these claims ignore the ability of individuals to economize such costs as well as the potential for outside assistance. Further, the costs of obtaining voter ID should be apportioned over multiple years and elections. Consequently, experts for the plaintiff greatly exaggerate the net costs of obtaining ID. Moreover, the estimated travel costs to obtain a free EIC are found o be higher for white voters versus black voters.

VIII. Voter ID as an Obstacle to Voting

128. Experts for the plaintiffs argue that i) a small increase in the cost of voting from SB 14 may have a large effect on turnout, and ii) the differential costs of obtaining ID for black and Hispanic voters will disproportionately suppress turnout among black and Hispanic voters in Texas. However, there are serious theoretical and empirical problems with both these claims.

The Calculus of Voting

129. One of the most elementary theories of voting posits that voters weigh the costs and benefits of voting when deciding whether to vote.[82] Several experts for the plaintiffs cite this Downsian theory of voting to support the contention that the costs of obtaining voter ID, ---even a free EIC--- , may have the effect of reducing turnout.[83]

130. However, there are two very different versions of this calculus of voting. The original formulation of the calculus of voting attributed to Downs may be summarized as follows:

A rational individual will choose to vote if $(p*B - C) > 0$, where:

p = the probability that a voter casts a decisive (e.g., deciding) vote,

B = the benefit to the voter of having her preferred candidate win, and

[82] Anthony Downs. 1957. *An Economic Theory of Democracy*. Harper (New York, NY).
[83] Barreto and Sanchez, p. 25; Bazelon, p. 6-7; Burden, p. 4-5; and Webster, p. 13.

C = the cost of voting for the individual.

In this simple characterization of the decision to vote, the only benefits of voting are the instrumental benefits of actually influencing the outcome of the election.

131. Even the originator of the simple calculus of voting theory, Anthony Downs, noticed that his economic theory of democracy was problematic; he describes his theory as "positive but not descriptive."[84] In other words, even Downs recognizes that his theory does not generate accurate predictions. As noted by Riker and Ordeshook, two political scientists who subsequently modify Downs original theory, "in science, one would expect to discard positive theories that are inadequate as descriptions."[85] Or, put another way, "Unfortunately for (the Downsian) theory, people do vote."[86]

132. There are different approaches for explaining the problems in the original Downsian model, but these approaches typically refer to the "Paradox of Voting." For example, in large-scale elections, the probability of casting a decisive vote (a vote that makes or breaks a tie) approaches zero.[87] This makes the expected instrumental benefit of voting (pB) very small. Consequently, a voter would choose to vote only if the costs of voting are even smaller. In this simple version of the calculus of voting, it must be true that only a very few people with the lowest costs of voting choose to vote. Further, even a small change in the cost of voting may reduce turnout dramatically because the expected instrumental benefits of voting are so small. However, this theory is falsified given that many people choose to vote, even in large-scale elections.[88] This phenomenon of people voting even though it is irrational to do so is sometimes referred to as the Paradox of Voting (or the Paradox of Voter Turnout).

132. There is yet another problem with the simple Downsian model. If only a very few people choose to vote, then the probability of casting a decisive vote becomes large. This inflates the expected instrumental benefits of voting (pB) and creates a situation where once again, many people wish to vote. But that in turn would cause the probability of casting a decisive vote to shrink back toward zero. In other words, the model may not have a stable equilibrium; that is, turnout is unpredictable, even in response to an increase in costs. This phenomenon is also sometimes referred to as the Paradox of Voting.[89]

133. A slightly more sophisticated version of the calculus of voting is typically attributed to Riker and Ordeshook.[90] They solve the paradox of voting by positing that there is some non-instrumental benefit

[84] Downs, p. 34.
[85] See fn. 2 in: Riker, William and Peter C. Ordeshook. 1968. "A Theory of the Calculus of Voting," *American Political Science Review*, 62(1): 25-42.
[85] Uhlaner, Carole. 1989. "Rational Turnout: The Neglected Role of Groups," *American Journal of Political Science*, 33(2): 390-422.
[87] E.g., for a recent calculation of this probability, see: Gelman, Andrew, Nate Silver and Aaron Edlin. 2012. "What is the Probability that Your Vote Will Make a Difference?" *Economic Inquiry*, 50(2): 321-326.
[88] E.g., Green, Donald and Ian Shapiro. 1994. *Pathologies of Rational Choice*. Yale university Press (New Haven, CT).
[89] The intransitivity of majority rule, an unrelated concept, is also sometimes referred to as the Paradox of Voting.
[90] Riker, William and Peter C. Ordeshook. 1968. "A Theory of the Calculus of Voting," *American Political Science Review*, 62(1): 25-42.

of voting; for example, a sense of a patriotic duty that confers a value on the act of voting even if it is futile. In this conception, the original Downsian model is modified to become:

A rational individual will choose to vote if $(p*B - C) + D > 0$, where:

p = the probability that a voter casts a decisive (e.g., deciding) vote,

B = the benefit to the voter of having her preferred candidate win,

C = the cost of voting for the individual, and

D = the non-instrumental value of voting.

134. Under this modified calculus of voting, it is possible for the instrumental value of voting to be essentially zero, but it would still be rational to vote, provided the intrinsic value of voting is sufficiently large. Importantly, under the modified theory of voting, a small change in the cost of voting is expected to result in some small reduction in turnout. This is because in general, not everyone will be just barely motivated to vote and therefore hypersensitive to small changes. Some individuals will have very large values of D and so will vote even when the costs of voting are large. Other individuals will have very small values of D and so will not vote even when the costs are quite small. Consequently, this modified theory does imply that the costs of voting are related to turnout, but the model does not generate the dramatic prediction that even a small change in the cost of voting will result in a large change in turnout.

135. The theory of voting has advanced some in the last several decades.[91] Even so, the *modified* Downsian model remains a useful tool for analyzing turnout in elections. Dr. Burden demonstrates the continued currency of the modified Downsian model with his discussion of the calculus of voting.[92]

136. However, other experts harken back to the defunct original Downsian model in order to argue that SB 14 may have a dramatic impact on turnout. For example, Bazelon states that: "Recent scholarship finds that 'Under [the "calculus of voting"], even small increases in the costs of voting can deter a person from voting, since the benefits of voting are so slight.'"[93] Barreto and Sanchez state that: "It is important to note that, with relatively low perceived benefits to voting among the electorate, even small increases to barriers to the ballot box can have a marked impact on turnout."[94] These claims are simply not supported by recent or modern scholarship on the determinants of voting.

137. Dr. Burden echoes these misguided claims by stating that:

[91] E.g., Riker, William and Peter C. Ordeshook. 1973. *An Introduction to the Positive Theory or Voting*. Prentice-Hall (Englewood Cliffs, NJ); Matsusaka, John. 1995. "Explaining Voter Turnout Patterns: An Information Theory," *Public Choice*, 84: 91-117; Dhillon, Amrita and Susana Peralta. 2002. "Economic Theories of Voter Turnout," *Economic Journal*, 112: F332-F352; Feddersen, Timothy . 2004. "Rational Choice Theory and the Paradox of Not Voting," *Journal of Economic Perspectives*, 18: 99-112; and Geys, Benny. 2006. "'Rational' Theories of Voter Turnout: A Review," *Political Studies Review*, 4: 16–35.

[92] Burden, p. 4-5.

[93] Bazelon, fn. 12, p. 6-7; Bazelon is in turn quoting from the recent voter ID verdict in Wisconsin, *Frank* v. *Walker* WL 1775432 at 17 (E.D. Wis. Apr. 29 2014).

[94] Barreto and Sanchez, p. 25.

> "This 'calculus of voting' framework suggests that for many individuals the decision to vote is made 'on the margins.' This is because the decision to vote is viewed as a 'low cost, low benefit calculation.' Small changes in costs may alter the likelihood dramatically."[95]

In this passage, Burden is quoting (and paraphrasing) John Aldrich, a political scientist who was writing in 1993.[96] However, Aldrich later argues in the same study that "... the low cost, low-benefit nature of the turnout decision really means that most of the action is, in fact, in the intrinsic values of voting per se (i.e., in C and D terms). After all, these apply directly to the voter, regardless." In other words, marginal voters are those individuals for whom (D - C) is very close to zero; therefore, a small change in C affects the turnout decision of only these marginal voters and so is unlikely to yield a "dramatic" change in turnout.

138. This subsequent argument by Aldrich regarding the importance of the non-instrumental or intrinsic benefits of voting (D) has been borne out by recent scholarship. A growing literature in political science analyzes field experiments on various get-out-the-vote (GOTV) tactics. In general, eligible voters treated with messages highlighting the civic duty to vote or activating social pressure to vote are significantly more likely to turnout compared to control groups of eligible voters.[97]

139. In the modern or modified version of the calculus of voting, whether an individual votes or not depends on whether (pB + D)>C. This differs from the original Downsian framework in that the total benefits of voting (pB + D) may be very large, even when the expected instrumental benefits (pB) are close to zero. Therefore, it no longer follows that a small change in the cost of voting will lead to a dramatic change in turnout. Consequently, Dr. Burden does not accurately convey the implications of the modified Downsian model that he articulates in his report.

140. By referencing Aldrich, Burden is reaching backward into the past and skipping over the fundamental lessons from much more recent empirical scholarship on the determinants of voting. In general, post-registration election procedures (e.g., early voting, extended polling hours, vote-by-mail, etc.) have fairly modest, insignificant or even perverse effects on voter turnout.[98] Judging from his

[95] Burden, p. 5.

[96] Aldrich, John. 1993. "Rational Choice and Turnout," *American Journal of Political Science*, 37(1): 246-278.

[97] E.g., Green, Donald and Alan Gerber. 2004. *Get Out the Vote*. The Brookings Institution (Washington, D.C.); Panagopoulos, Costas, Christopher W. Larimer and Meghan Condon. 2014. "Social Pressure, Descriptive Norms, ad Voter Mobilization," *Political Behavior*, 36: 451-469.

[98] E.g., Gronke, Paul, Eva Galannes-Rosenbaum and Peter A. Miller. 2007. "Early Voting and Turnout," *PS: Political Science & Politics*, 40(4): 639-645; Fitzgerald, Mary. 2005. "Greater Convenience But Not Greater Turnout: The Impact of Alternative Voting Methods on Electoral Participation in the United States," *American Politics Research*, 33(6): 842-867; Primo, David, Matthew Jacobsmeier and Jeffrey Milyo. 2007. "Estimating the Impact of State Policies and Institutions with Mixed-Level Data," *State Politics and Policy Quarterly*, 7(4): 446-459; and Traugott, Michael W. 2004, "Why Electoral Reform Has Failed: If You Build It, Will They Come?" In: *Rethinking The Vote: The Politics and Prospects of American Election Reform*, eds. Ann N. Crigler, Marion I. Just, and Edward J. McCaffery,

published research, Dr. Burden is well aware of this literature, as well as the importance of non-instrumental determinants of voting.[99] Therefore it is quite puzzling that he would make an argument that is based on the discredited early Downsian model of voting; in doing so, Burden is simply not making use of his expertise.

141. Given the recent and well-known findings in the scholarly literature regarding the weak effects of many post-registration election procedures, it is also puzzling that Barreto and Sanchez state that the simple version of the Downsian theory "... has been *verified* over time by political scientists who have identified institutional constraints (e.g. registration and voting requirements) as the chief source of cost imposition to voters. (emphasis added)"[100] This statement is highly misleading and not consistent with their expertise as scholars of American politics.

142. Furthermore, Barreto and Sanchez note that state voter registration laws impose costs on voters and therefore assert that such laws have large negative effects on turnout.[101] In support of this claim, they cite Piven and Cloward (1988) and Rosenstone and Wolfinger (1978).[102] However, recent empirical studies find much more modest effects of voter registration on turnout.[103]

Differential Costs of Voting and Turnout

143. Yet another errant claim made by several experts for the plaintiffs is that increased costs of voting for black and Hispanic voters necessarily depresses turnout for these groups relative to non-Hispanic white voters.[104] The modified version of the Downsian model generates an ambiguous prediction regarding how an increase in costs will affect different groups of voters. I demonstrate this with two hypothetical examples:

144. Hypothetical Example 1:

Oxford University Press (New York, NY). However, also see: McNulty, John E., Conor M. Dowling and Margaret H. Ariotti. 2014. "Driving Saints to Sin: How Increasing the Difficulty of Voting Dissuades Even the Most Motivated Voters," *Political Analysis*, 17 (4): 435-455.

[99] Burden, Barry, David T. Canon, Kenneth Mayer, and Donald P. Moynihan. 2014. "Election Laws, Mobilization, and Turnout: The Unanticipated Consequences of Election Reform," *American Journal of Political Science*, 58(1): 95-109.

[100] Barreto and Sanchez, p. 26.

[101] Barreto and Sanchez, p. 26;

[102] Piven, Frances Fox, and Richard A. Cloward. 1988. *Why Americans Don't Vote*. Pantheon Books (New York); and Rosenstone, Steven and Raymond Wolfinger. 1978. "The Effect of Registration on Voter Turnout." *American Political Science Review*, 72 (1), 22-45.

[103] Ansolabehere, Stephen and David Konisky. 2006. "The Introduction of Voter Registration and Its Effect on Turnout," *Political Analysis*, 14 (1): 83-100; and Burden, Barry and Jacob Neiheisel. 2013. "Election Administration and the Pure Effect of Voter Registration on Turnout," *Political Research Quarterly*, 66(1): 77-90.

[104] Burden, p. 29; Burton, p. 44.; and Lichtman, p. 9. For a similar claim, see Ansolabehere, p. 51.

(a) First, consider two groups of voters with identical distributions of the total benefits of voting (pB + D), but different costs of voting. Let these two groups be called white and black, and let the costs of voting be higher for black voters than white voters. Further assume that the benefits and costs of voting are always constrained to values such that aggregate turnout is positive.

(b) For any distribution of the benefits of voting throughout the population, this will imply that white voters have a higher turnout than black voters.

(c) For sake of exposition, now consider an increase in the costs of voting that applies to both groups equally. Again, for any distribution of the benefits of voting, this implies that turnout goes down in each group. However, whether the number of voters adversely effected is greater for the white group or black group depends on the distribution of total benefits and the particular values for the costs of voting. Likewise the percentage of voters adversely affected may be higher or lower for the black group compared to the white group.

(d) Finally, consider a case where the distribution of benefits and costs are such that an equal increase in costs results in a greater (percentage) reduction in turnout for white voters. If instead the costs of voting increase more for the black group than the white group (but costs are identical within groups), it is indeed possible that the (percentage) reduction in turnout will be greater for black voters than white voters. However, there will be a range of changes in the differential costs of voting that yield a greater (percentage) reduction in turnout for white voters compared to black voters.

145. Hypothetical Example 2:

(a) First, consider two groups of voters with different distributions of the total benefits of voting (pB + D), but identical costs of voting. Let these two groups be called white and black, and let the distribution of benefits be such that median value is greater for the white group than the black group. Further assume that the benefits and costs of voting are always constrained to values such that aggregate turnout is positive.

(b) Now, whether turnout is higher for the white group or the black group depends on the particular distribution of total benefits for each group.

(c) Consider a case such that turnout is lower for the black group than the white group. Now consider a change in the costs of voting that is identical for all voters. Once again, it is possible for the number or percentage of affected black voters to be greater or lower compared to white voters. The result depends on the particular distributions of total benefits and the particular values of the costs of voting.

(d) Next consider a case such that the identical increase in costs yields a greater (percentage) reduction in turnout for the white group compared to the black group. If instead, the increase in costs is identical within groups but higher for the black group, then it is possible that this differential change in costs results in a greater (percentage) reduction in turnout for black voters compared to white voters.

However, there will be a range of changes in the differential costs of voting that yield a greater (percentage) reduction in turnout for white voters compared to black voters.

146. These two examples are sufficient to demonstrate that the Downsian theory of voting does not generate the predictions that experts for the plaintiffs assume. In particular, the existence of higher turnout among non-Hispanic white voters does not imply lower costs of voting for that group compared to black voters or Hispanic voters. Further, an identical increase in the costs of voting for all voters does not necessarily lead to a greater reduction in turnout for those groups relative to white voters. Finally, even a differential increase in the costs of voting for black and Hispanic voters does not necessarily lead to a greater reduction in turnout for those groups relative to non-Hispanic white voters.

147. As a group, experts for the plaintiffs devote a great deal of effort to attempting to demonstrate that SB 14 imposes higher costs of voting on black and Hispanic voters than non-Hispanic voters. But the theory of voting that these experts rely on does not predict that SB 14 will adversely affect turnout among black and Hispanic voters more so than for white voters. Even within the framework established by the experts for the plaintiffs, the theoretical effects of SB 14 on relative turnout across groups are ambiguous.

Implications of Voter ID for Voter Turnout

148. As a group, experts for the plaintiffs assume that SB 14 only has the effect of raising the costs of voting. It has been demonstrated that even in this scenario, the question of whether voter ID laws like SB 14 have the effect of suppressing minority votes in some disproportionate manner is an empirical one. Turnout among black or Hispanic voters may be more or less affected than turnout among non-Hispanic white voters. However, if voter ID laws only have the effect of increasing costs, then turnout is expected to go down for all groups.

149. The assumption that voter ID laws only increase costs is unwarranted. Voter ID laws like SB 14 may affect the intrinsic benefits of voting in multiple ways. For example, to the extent that SB 14 increases confidence in the integrity of the voting process, or otherwise heightens the salience of voting, the non-instrumental benefits of voting will go up, as well.[105] Further, voter ID laws may motivate political entrepreneurs and advocacy groups to engage in additional voter mobilization efforts. Such efforts may include voter education and encouragement, or efforts to reduce the costs of voting by offering rides to the polls or to the DPS (for voters needing an EIC). In fact, as one of the experts for the plaintiffs, SB 14 mandates a statewide voter education campaign on voter ID.[106] Consequently, the effect of SB 14 on the net benefits of voting is itself an empirical question.

[105] E.g., Citrin, Jack, Donald Green and Morris Levy. 2014. "The Effects of Voter ID Notification on Voter Turnout: Results from a Large-Scale Field Experiment," *Election Law Journal*, 13(2): 228-242; and Milyo, Jeffrey. 2007. "The Effects of Photographic Identification on Voter Turnout in Indiana: A County-Level Analysis," Institute of Public Policy Report 10-2007, University of Missouri (Columbia, MO).
[106] Report of T. Ransom Cornish, p. 7.

150. Citrin et al (2014) conduct a unique field experiment that confirms the potential positive effects of voter ID laws on turnout.[107] The experiment was conducted in 2012 in precincts along the Tennessee-Virginia border with heavily African American populations. The study compares whether individuals treated with notices regarding the need for voter ID at the polls were more or less likely to vote compared to a control group. The authors find "no evidence that calling attention to voter identification requirements dissuades voters from voting." Further, the study finds that "informing low-propensity voters of new identification requirements raises turnout by approximately one percentage point." Moreover, "Messages providing details about ID requirements and offering to help recipients obtain acceptable ID appear somewhat more effective than messages only pointing out the need to bring proof of ID." Interestingly, the positive treatment effects on turnout are larger in Tennessee, a state that has a so-called strict photo ID law.

151. There is a growing empirical literature that examines the effects of voter ID laws on voter turnout. Overall, the findings in this literature are decidedly mixed.[108] However, several recent studies report that overall voter turnout rates are either unaffected (i.e., no statistically significant difference) or positively affected by state voter ID laws.[109] Of note, two such studies were conducted by experts for the plaintiffs, but not cited by these experts in their reports.[110] In addition, Dr. Ansolabehere has conducted related research on the incidence of people who cite ID problems as the reason for not voting.[111] He has concluded that: "Voter ID does not appear to present a significant barrier to voting."[112] There is also mixed evidence regarding any differential effect of state voter ID laws on black and Hispanic voters.[113] For example, Alvarez et al. emphasize:

[107] Citrin et al. 2014.

[108] E.g. see the recent reviews in: Citrin et al. 2014; and Hershey, Marjorie Randon. 2009. "What We Know about Voter-ID Laws, Registration, and Turnout," *PS: Political Science*, 42(1): 87-91.

[109] E.g., Ansolabehere, Stephen 2009. "Effects of Identification Requirements on Voting: Evidence from the Experiences of Voters on Election Day," *PS: Political Science*, 42(1): 127-130; Mycoff, Jason D., Michael W. Wagner, and David C. Wilson. 2009. "The Empirical Effects of Voter-ID Laws: Present or Absent," *PS: Political Science*, 42(1): 121-126; Larocca, Roger and John S. Klemanski. 2011. "U.S. State Election Reform and Turnout in Presidential Elections," *State Politics and Policy*, 11(1): 76-101; and Alvarez, Michael R., Delia Bailey and Jonathan Katz. 2008. "The Effect of Voter Identification Laws on Turnout," Voting Technology Project Working Paper #57, Caltech (Pasadena, CA).

[110] Erickson, Robert and Lorraine C. Minnite. 2009. "Modeling Problems in the Voter Identification ---Voter Turnout Debate," *Election Law Journal*, 8(2): 85-101; and Burden, Barry, C., David T. Canon., Kenneth R. Mayer and Donald P. Moynihan. 2014. "Election Laws, Mobilization, and Turnout: The Unanticipated Consequences of Election Reform," *American Journal of Political Science*, 58(1): 95-109.

[111] Ansolabehere, Stephen 2009. "Effects of Identification Requirements on Voting: Evidence from the Experiences of Voters on Election Day," *PS: Political Science*, 42(1): 127-130; and Ansolabehere, Stephen. 2008. "Access Versus Integrity in Voter Identification Requirements," *New York University Annual Survey of American Law*, 63(4): 613-630.

[112] Ansolabhere, 2009. P 129.

[113] E.g., Alvarez, Michael R., Delia Bailey and Jonathan Katz. 2008. "The Effect of Voter Identification Laws on Turnout," Voting Technology Project Working Paper #57, Caltech (Pasadena, CA); and Hood, M.V. and Charles S. Bullock. 2012. "Much Ado about Nothing? An Empirical Assessment of the Georgia Voter Identification Statute," *Legislative Studies Quarterly*, 12(4): 394-414. Also see, Ansolabehere, Stephen. 2008. "Access Versus Integrity in Voter Identification Requirements," *New York University Annual Survey of American Law*, 63(4): 613-630.

> "This is an important result. Controlling for the factors usually seen in models of voter participation, especially education and income, we see no evidence that strict voter identification requirements are racially discriminatory."[114]

Consequently, there is no strong and consistent evidence that state voter ID laws reduce overall turnout, or that such laws suppress black and Hispanic turnout relative to Non-Hispanic white turnout.

152. It is instructive to note that until the recent flurry of studies on state voter ID laws, political scientists have shown remarkably little interest in voter identification as a determinant of turnout. For example, there is a longstanding scholarly empirical literature which examines turnout differences across countries, but ignores voter identification as a possible determinant of such differences.[115] In fact, in a recent meta-analysis of 83 such studies, differences in voter identification are not even mentioned as factors that might influence differences in turnout across countries.[116]

153. It is telling that none of the 17 experts for the plaintiffs cite the empirical literature on the effects of state voter ID laws on voter turnout. After all, "The true measure of the effects of ID requirements lies in the rate at which such rules exclude or prevent people from voting."[117] The fact that experts for the plaintiffs do not discuss these studies corroborates my own view that the weight of evidence does not support their claim that voter ID laws impose only costs on voters and therefore decrease turnout (especially among black and Hispanic voters).

154. It is also telling that none of the 17 experts for the plaintiffs attempt to conduct their own original analysis of the effects of state voter ID laws on voter turnout. For example, several experts work with multiple years of data from the ACS, CPS or CCES, but none of them attempt to estimate the treatment effects of "strict photo ID" laws on voter turnout (and differences in turnout by race and ethnicity). Dr. Ansolabehere works extensively with multiple years of Catalist data, but does not attempt to conduct a similar study of the treatment effects of voter ID on turnout. Moreover, the fact that there have been several elections in Texas since the implementation of SB 14 provides a potential "natural experiment" for analyzing the impact of SB 14 on voter turnout in Texas. Further, because Catalist data are updated continuously, Ansolabehere might have even conducted a before-and-after analysis of the effects of SB 14 in Texas on voter turnout and differences in turnout by race and ethnicity. Similarly, Barreto and Sanchez included questions about voting before and after the implementation of SB 14 in their survey, but they do not attempt to analyze the effect of SB 14 on turnout or differences in turnout by race and ethnicity.

[114] Alvarez et al. 2008. P. 18.
[115] E.g., Powell, G. Bingham. 1986. "American Voter Turnout in Comparative Perspective," *American Political Science Review*, 80: 17-44.; and Blaise, Andre. 2006. "What Affects Voter Turnout?" *Annual Review of Political Science*, 9: 111-125.
[116] Gey, Benny 2006. "Explaining Voter Turnout: A Review of Aggregate-Level Research," *Electoral Studies*, 25(4): 637-663.
[117] Ansolabehere, 2008, p. 623.

155. It is my expectation that such original data analyses will not yield convincing evidence that is consistent with the plaintiffs' arguments. The fact that multiple experts have the requisite data and skills to conduct such tests but choose not to do so suggests that they concur in my judgment. The fact that none of the experts even cite existing relevant studies of the effects of voter ID on turnout is further corroboration that a careful examination of the evidence does not support their arguments.

Summary of Section VIII: Experts for the plaintiffs argue that a small increase in the cost of voting from SB 14 will have the effect of suppressing turnout among blacks and Hispanics in Texas. However, the primary support for these claims is a defunct theory of voting from more than 50 years ago. I demonstrate that the theoretical effects of SB 14 on turnout are ambiguous, so that the claims regarding turnout can only be evaluated empirically. However, the most relevant empirical literature on the effects of voter ID laws and turnout provides no strong or consistent support for these claims. Instead, recent evidence suggests that state voter ID laws may have a mobilizing effect on voter turnout, even or especially among minority voters. The experts for the plaintiffs fail to acknowledge this directly relevant scholarly literature. Experts for the plaintiffs also fail to conduct any systematic statistical analysis of the treatment effects of state voter ID laws on turnout in their reports. For example, there have been several elections since the implementation of SB 14 in Texas; these elections provide a "natural experiment" for analyzing the impact of SB 14 on voter turnout. Taken together, these failures call into question the reliability of the experts for the plaintiffs.

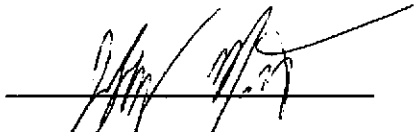
IX. Racially Discriminatory Intent

156. Several experts for the plaintiff argue that SB 14 was passed with the intent to substantially and disproportionately suppress turnout among otherwise eligible black and Hispanic voters.[118] The limited time available to me does not permit a detailed examination of the theory and evidence offered to support this claim. However, my analysis in this report is sufficient to severely undercut the arguments made by experts for the plaintiffs. Even so, I have identified several problems with the arguments made by experts for the plaintiffs with regard to this claim. In my opinion, the arguments made in support of this claim are particularly tendentious and ignore the value of demagoguery on voter ID for Democratic politicians and others. Further, in my opinion, experts for the plaintiffs ignore the most important factor behind the passage of SB 14, its popularity. In my opinion, it is well-known that voter ID laws are in general popular with the general public, although it is possible that support among the black and Hispanic population has fallen off owing to the exaggerated arguments made in opposition to voter ID.

[118] E.g, see the reports by Burden, Burton, Davidson, Korbel, and Lichtman.

I declare under penalty of perjury under the laws of the United States that the foregoing is true and correct to the best of my knowledge.

Executed on August 15, 2014.



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UNITED STATES DISTRICT COURT
SOUTHERN DISTRICT OF TEXAS
CORPUS CHRISTI DIVISION

MARC VEASEY, *et al*,	§	
	§	
Plaintiffs,	§	
VS.	§	CIVIL ACTION NO. 2:13-CV-00193
	§	
RICK PERRY, *et al*,	§	
	§	
Defendants.	§	

DISCLOSURE UNDER FEDERAL RULES OF CIVIL PROCEDURE/BACKGROUND OF WITNESS

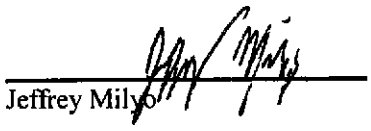
I have written 40 articles, notes and reviews, contributions to edited volumes and other reports in the last ten (10) years, and I have testified in one case in the past four (4) years which is attached to this disclosure. I have agreed to provide my services in this matter for a fee of $65,000 plus an hourly rate of $400.00 per hour for deposition and court time. I have spent approximately 175 hours on this case so far. I anticipate that I will spend an additional 10 hours to complete my work including preparing for and providing my deposition and preparing for and providing my trial testimony. I have been paid a total of $15,000.00 to date.

The facts and data considered by me in forming the opinion expressed in my report are included in the report together with citations, and other research performed by me and/or are based on my approximately 20 years of experience as a professor and scholar of American political economics.

I have submitted an expert report and was deposed in connection to a dispute in federal court regarding Washington State's disclosure requirements for groups engaged in grass roots issue advocacy (*Many Cultures, One Message, et al., v. Clements, et al.*); my report analyzed the impact of Washington's Grass Roots Lobbying Law (Wash. Rev. Code § 42.17.200) on the ability of citizens to freely exercise their First Amendment rights to speak, associate, assemble and petition government.

I declare under penalty of perjury under the laws of the United States that my report is true and correct to the best of my knowledge.

Signed this the 15th day of August, 2014.



Jeffrey Milyo

ATTACHMENT

Cases in which I have Testified and or Given Depositions in the Past Five Years:

Many Cultures, One Message, et al., v. Clements, et al., Washington Western District Court (Case Number 3:2010cv05253). Testimony at deposition.

Motivated Skepticism in the Evaluation of Political Beliefs

Charles S. Taber Stony Brook University
Milton Lodge Stony Brook University

We propose a model of motivated skepticism that helps explain when and why citizens are biased-information processors. Two experimental studies explore how citizens evaluate arguments about affirmative action and gun control, finding strong evidence of a prior attitude effect *such that attitudinally congruent arguments are evaluated as stronger than attitudinally incongruent arguments. When reading pro and con arguments, participants (Ps) counterargue the contrary arguments and uncritically accept supporting arguments, evidence of a* disconfirmation bias. *We also find a* confirmation bias*—the seeking out of confirmatory evidence—when Ps are free to self-select the source of the arguments they read. Both the confirmation and disconfirmation biases lead to* attitude polarization*—the strengthening of t_2 over t_1 attitudes—especially among those with the strongest priors and highest levels of political sophistication. We conclude with a discussion of the normative implications of these findings for rational behavior in a democracy.*

So convenient a thing is it to be a rational creature, since it enables us to find or make a reason for everything one has a mind to.

Ben Franklin

Physicists do it (Glanz 2000). Psychologists do it (Kruglanski and Webster 1996). Even political scientists do it (cites withheld to protect the guilty among us). Research findings confirming a hypothesis are accepted more or less at face value, but when confronted with contrary evidence, we become "motivated skeptics" (Kunda 1990), mulling over possible reasons for the "failure," picking apart possible flaws in the study, recoding variables, and only when all the counterarguing fails do we rethink our beliefs. Whether this systematic bias in how scientists deal with evidence is rational or not is debatable, though one negative consequence is that bad theories and weak hypotheses, like prejudices, persist longer than they should.

But what about ordinary citizens? Politics is contentious (Newman, Just, and Krigler 1992). In the marketplace of ideas, citizens are confronted daily with arguments designed to either bolster their opinions or challenge their prior beliefs and attitudes (Gamson 1992). To the extent that ordinary citizens act similarly to scientists the consequences would be similar—hanging on to one's beliefs and attitudes longer and stronger than warranted. Of course, it would be foolish to push this analogy too hard since scientific practice has such built-in safeguards as peer review and double-blind experiments to prevent bad ideas from driving the good ones out of the marketplace.

Ideally, one's prior beliefs and attitudes—whether scientific or social—should "anchor" the evaluation of new information and then, depending on how credible is some piece of evidence, impressions should be adjusted upward or downward (Anderson 1981). The "simple" Bayesian updating rule would be to increment the overall evaluation if the evidence is positive, decrement if negative. Assuming one has established an initial belief (attitude or hypothesis), normative models of human decision making imply or posit a two-step updating process, beginning with the collection of belief-relevant evidence, followed by the integration of new information with the prior to produce an updated judgment. Critically important in such normative models is the requirement that the collection and integration of new information be kept independent of one's prior judgment (see Evans and Over 1996).

In this article we report the results of two experiments showing that citizens are prone to overly accommodate

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American Journal of Political Science, Vol. 50, No. 3, July 2006, Pp. 755–769

ISSN 0092-5853

supportive evidence while dismissing out-of-hand evidence that challenges their prior attitudes. On reading a balanced set of pro and con arguments about affirmative action or gun control, we find that rather than moderating or simply maintaining their original attitudes, citizens—especially those who feel the strongest about the issue and are the most sophisticated—strengthen their attitudes in ways not warranted by the evidence.

A Theory of Motivated Political Reasoning

Our starting premise (following Kunda 1987, 1990) is that *all reasoning is motivated*. While citizens are always constrained in some degree to be accurate, they are typically unable to control their preconceptions, even when encouraged to be objective. This tension between the drives for accuracy and belief perseverance underlies all human reasoning. Keeping it simple and focusing on reasoning about things political, citizens are goal oriented (Chaiken and Trope 1999). Their motives fall into two broad categories: *accuracy goals*, which motivate them to seek out and carefully consider relevant evidence so as to reach a correct or otherwise best conclusion (Baumeister and Newman 1994; Fiske and Taylor 1991), and *partisan goals*, which motivate them to apply their reasoning powers in defense of a prior, specific conclusion (Kruglanski and Webster 1996). In our theory, partisan goals and subsequent selective information processing are driven by automatic affective processes that establish the direction and strength of biases (Lodge and Taber 2005; Taber, Lodge, and Glathar 2001). Sociopolitical concepts are "hot" for most people, so that associated attitudes come to mind automatically along with, indeed prior to, semantic information. One's likes or dislikes for Hillary Clinton, for example, are aroused even before conscious awareness of her identity and other semantic associations—that she is a Democratic senator, a woman, and a former first lady (Morris et al. 2003). These "hot cognitions," in our view, motivate the partisan goals that drive normatively suspect selectivity in subsequent information processing.

Surprisingly, given the widespread acceptance of selective attention, exposure, and judgment processes throughout the social sciences, the empirical evidence from social psychology is far more mixed and qualified than is often believed. The empirical status of selective attention and, in particular, selective exposure can best be characterized as uncertain (Abelson et al. 1968; Eagly and Chaiken 1993, 1998; Freedman and Sears 1965; Frey 1986; Greenwald et al. 2002; Kunda 1990; Lord 1992; Pomerantz, Chaiken, and Tordesillas 1995; Wicklund and Brehm 1976).

Selective information processes are particularly important because of their impact on subsequent attitudes and behavior and because of their implications for the distribution of aggregate public opinion (Zaller 1992). Theoretically, we should expect *attitude polarization*: those holding strong prior attitudes become attitudinally *more* extreme on reading pro and con arguments because they assimilate congruent evidence uncritically but vigorously counterargue incongruent evidence (Ditto and Lopez 1992; Rucker and Petty 2004). Unfortunately, the empirical pedigree of this classic expectation is even more dubious than the various selectivity hypotheses. The most cited support for attitude polarization comes from the Lord, Ross, and Lepper (1979) study of attitudes toward the death penalty, but even this evidence is unconvincing because it is based on subjective rather than direct measures of polarization. Rather than comparing t_1 and t_2 measures of attitudes, Lord and his colleagues asked subjects to report *subjectively* whether their attitudes had become more extreme after evaluating pro and con evidence on the efficacy of capital punishment. Moreover, numerous attempts to replicate polarization using direct t_1 and t_2 measures of social and political attitudes have failed (e.g., Kuhn and Lao 1996; Miller et al. 1993; Pomerantz, Chaiken, and Tordesillas 1995).

We believe that attitude polarization has been elusive in psychological research for at least two reasons. First, we suspect that the arguments and evidence used in many of these studies failed to arouse sufficient partisan motivation to induce much biased processing. Since most of the work in the cognitive dissonance tradition did not consider the strength of prior affect to be critical, little effort was made to create stimuli that would elicit strong affective responses. Some research, for example, relied on syllogistic arguments that are hard to understand (e.g., Oakhill and Johnson-Laird 1985); other research used oversimplified policy statements comprised of a single stylized premise and conclusion (Edwards and Smith 1996). Selective biases and polarization, we believe, are triggered by an initial (and uncontrolled) affective response; by contrast, most of the work on selectivity and polarization in social psychology uses rather *cold* arguments and rests on theories of cold cognition (most commonly, dissonance theory).

In our motivated reasoning experiments, we use statements and arguments taken directly from political interest groups, which are far more contentious and more in line with contemporary political discourse (Ailes 1995; Ansolabehere and Iyengar 1995); these arguments often

generate strong affective responses (see Figure 2, below, for an example argument).

The second and more difficult problem for those seeking to find attitude polarization is the weak measurement of attitude change and the severe scale constraints that ensue. Researchers have typically (e.g., Edwards and Smith 1996) relied on a single item, presented pre- and posttask, to measure attitude extremity and change. The problem, of course, in addition to the weak reliability of a single item, is that while the theory holds that those with the most extreme attitudes are the most prone to become even more extreme, detecting any such change is thwarted by the upper and lower bounds of the scale and by regression to the mean. We employ a six-item additive scale to measure attitudes at t_1 and t_2, which improves measurement reliability and reduces the number of respondents at or near the scale limits at t_1.

Based on our theory of affect-driven motivated reasoning, we posit three mechanisms of partisan or biased processing:

- *H1*: a *prior attitude effect*, whereby people who feel strongly about an issue—even when encouraged to be objective and leave their preferences aside—will evaluate supportive arguments as stronger and more compelling than opposing arguments;
- *H2*: *a disconfirmation bias*, such that people will spend more time and cognitive resources denigrating and counterarguing attitudinally incongruent than congruent arguments; and
- *H3*: *a confirmation bias*, such that when free to choose what information they will expose themselves to people will seek out confirming over disconfirming arguments.

Because each of these mechanisms deposits more supporting than repudiating evidence in mind, we predict

- *H4*: *attitude polarization*, whereby attitudes will become more extreme, even when people have been exposed to a balanced set of pro and con arguments.

Our theory, at first glance, might suggest we are arguing that people are closed-minded, consciously deceiving themselves to preserve their prior beliefs. On the contrary, a key argument we make (Lodge and Taber 2005; Taber 2003) is that people are largely unaware of the power of their priors. It is not that they openly lie to themselves. Rather, they try hard to be fair-minded or at least preserve the "illusion of objectivity" (Pyszczynski and Greenberg 1987), but they are frequently unable to do so. On the other hand, as the persuasion literature clearly shows (Petty and Wegener 1998) and as attested to in the study of voting behavior (Aldrich, Sullivan, and Borgida 1989; Rabinowitz and MacDonald 1989), even those committed to their positions can be persuaded by strong and credible counterevidence (Festinger 1957). But the research we report suggests that, once attitudes have become crystallized, persuasion is difficult. Asymmetrical skepticism—as would be reflected in the type of thoughts that come to mind as we read pro and con arguments—deposits in mind all the evidence needed to justify and bolster our priors with a clear conscience (Ditto et al. 1998).

Being a motivated reasoner takes effort (Lavine, Borgida, and Sullivan 2000; Pomerantz, Chaiken, and Tordesillas 1995); hence we expect Hypotheses 1–4 to be conditional on the strength of one's prior attitude (motive) and on one's level of political sophistication (opportunity).

- *H5*: an *attitude strength effect*, such that those citizens voicing the strongest policy attitudes will be most prone to motivated skepticism; and
- *H6*: a *sophistication effect*, such that the politically knowledgeable, because they possess greater ammunition with which to counterargue incongruent facts, figures, and arguments, will be more susceptible to motivated bias than will unsophisticates.

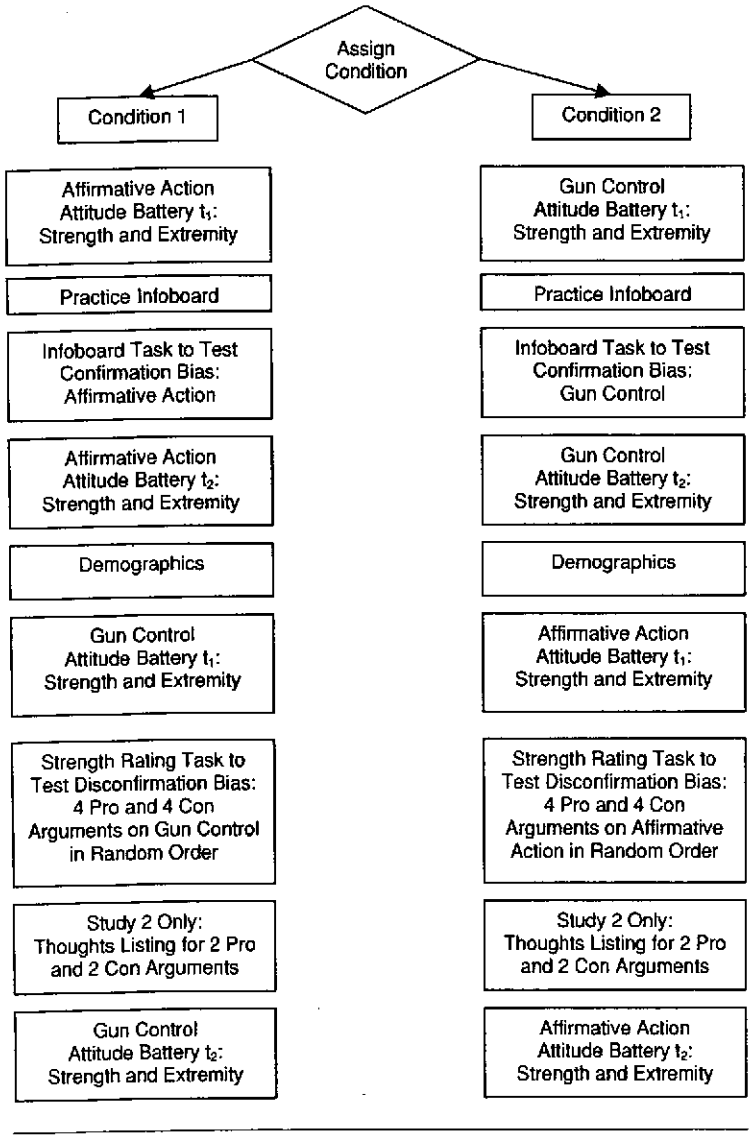
Experiments on the Mechanisms of Biased Reasoning

Two experiments were carried out to test these six hypotheses.[1] Participants (Ps) were recruited from introductory political science courses at Stony Brook University. Their participation, for which they received course credit, consisted of a single session lasting less than one hour (Study 1: N = 126, 59 male, 70 white, 64 Democrat, 34 Republican; Study 2: N = 136, 68 male, 64 white, 61 Democrat, 21 Republican). Since the two experiments share the same basic design, differing in but one manipulation, we will describe them together (Figure 1).

On entering the laboratory, Ps were seated individually at computers in separate experimental rooms and instructed that they would take part in a study of public opinion. Their first task was to evaluate a number of contemporary political issues, among them a battery of items tapping their attitudes on either affirmative action or gun control (with the sample split into two conditions by random assignment). These attitude measures included

[1]Since several independent variables are measured rather than manipulated (prior attitude and sophistication), this is more properly thought of as a quasi-experimental design.

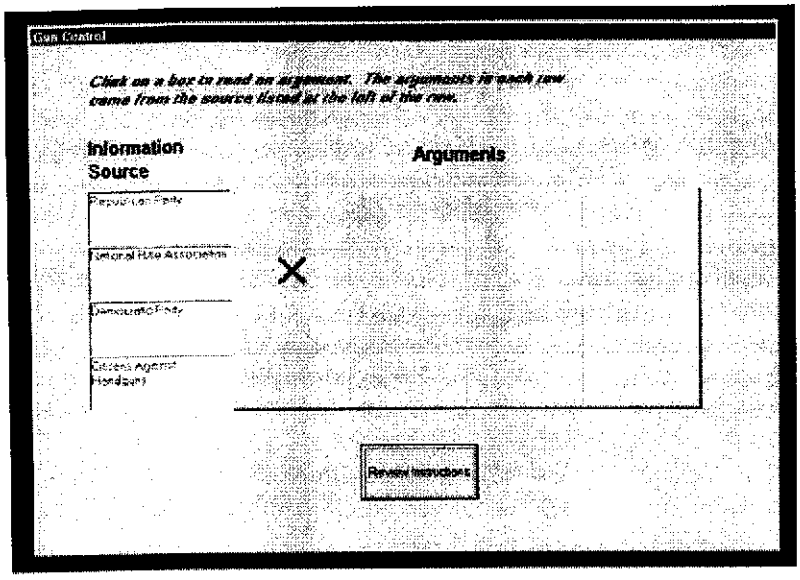
FIGURE 1 Experimental Design



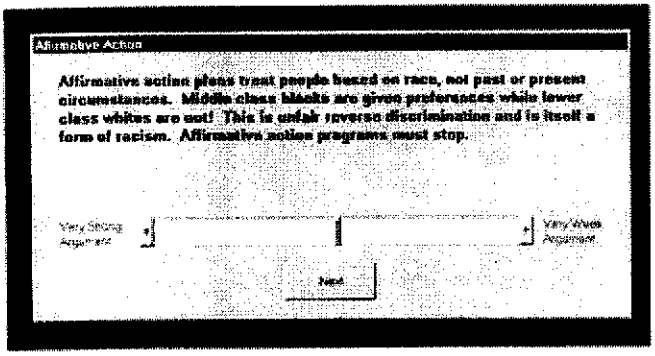
four items designed to measure *attitude strength* (recorded on 100 point-sliding response scales) and six items that measure *attitude position* (9-point agree/disagree Likert items; see http://www.stonybrook.edu/polsci/ctaber/taberlodgeajps05.pdf for the items). Additive scales were constructed for both variables and rescaled to [0,1] with responses below 0.5 indicating "weak" or "con," respectively.[2] In keeping with prior research (for an overview, see Petty and Krosnick 1995), strength and position are independent attitudinal dimensions such that some respondents took extreme positions on the issues without feeling strongly about those positions, and some moderates rode the fence with conviction.

[2]Both scales are reliable. The attitude extremity scale produced the following standardized item alphas, with subscripts indicating prior or posterior measurement: for affirmative action in Study 1, $\alpha_1 = .80$ and $\alpha_2 = .87$; for gun control in Study 1, $\alpha_1 = .75$ and $\alpha_2 = .72$; for affirmative action in Study 2, $\alpha_1 = .82$ and $\alpha_2 = .93$; for gun control in Study 2, $\alpha_1 = .77$ and $\alpha_2 = .89$. The comparable alphas for the attitude strength scale were: for affirmative action in Study 1, $\alpha_1 = .90$ and $\alpha_2 = .92$; for gun control in Study 1, $\alpha_1 = .91$ and $\alpha_2 = .94$; for affirmative action in Study 2, $\alpha_1 = .93$ and $\alpha_2 = .93$; for gun control in Study 2, $\alpha_1 = .91$ and $\alpha_2 = .90$. The distributions of responses were skewed slightly toward support for affirmative action (median extremity score: .56) and strongly toward support for gun control (median extremity score: .67). Correlations between (folded) extremity and strength did not exceed .20.

FIGURE 2 The Primary Experimental Tasks (a) Information Board (b) Argument Strength Rating Box



(a)



(b)

After completing the attitude battery for the first time, Ps practiced using an information board designed to track their search for pro or con information about affirmative action (or gun control in the other condition). They were instructed to view information in an evenhanded way so that they could explain the issue to other students (such instructions enhance accuracy motivation and work against partisan motivation). Our information board presented a matrix of 16 hidden policy arguments (rows and columns randomized), which Ps could only view by clicking on a button in the matrix (see Figure 2a). Rows of arguments were labeled with a known source, so that participants knew which hidden arguments would favor and which would oppose the issue; moreover, Ps were explicitly told each group's position on the issue as part of their instructions and were subsequently tested to make sure they understood. Ps viewed eight arguments with no time limit, but could not view the same argument a second time. The computer recorded the order and viewing time for each argument selected. This task provides our test for the *confirmation bias*—the prediction that people, especially those who feel the strongest and know the most, will seek out confirmatory evidence and avoid what they suspect might be disconfirming evidence. All Ps then completed

the same attitude battery a second time (so as to measure $t_1 \rightarrow t_2$ attitude change).

A substantial set of demographic questions followed the information board task, including all the usual suspects: PID, ideological self-placement, race, gender, etc., and most important for our purposes, a 17-item general political knowledge scale (asking, e.g., "What proportion of Congress is needed to override a presidential veto?"). Our measure of political sophistication is the proportion of correct responses, which for many subsequent analyses we subject to a tertile split (so we may contrast the top and bottom thirds of the sample).

The second part of the experiments, testing for a *disconfirmation bias*, began with a third administration of the attitude battery as described above, but with the issues flipped across conditions, so that Ps who received affirmative action for the information board task now rated gun control, and vice versa. Ps were then asked to rate the strength of eight arguments, four pro and four con (presented sequentially in random order; see Figure 2b for a sample strength rating box). Again, Ps were instructed to be evenhanded and told that they would be asked to explain the controversy to other students (to maximize accuracy goals). This argument-strength rating task was followed by the posttest attitude battery and a recognition memory test. In addition—this the only significant difference between Studies 1 and 2—Ps in Study 2 were asked to list their thoughts for two pro and two con affirmative action or gun control arguments.

The arguments used in our experiments were drawn from print and online publications of real issue-relevant interest groups (including the NRA, NAACP, Brady Anti-Handgun Coalition, and the platforms of the Republican and Democratic parties). To control for such alternative explanations for processing bias as the "argument length = strength" or "complexity = strength" heuristics (Cobb and Kuklinski 1997; Petty and Cacioppo 1981), the arguments were edited such that they had similar complexities (length of sentence, average number of syllables, words per sentence, sentences per argument, reading level, and so forth) and were pretested on student samples (see the full set of arguments at http://www.stonybrook.edu/polsci/ctaber/taberlodgeajps05.pdf).

Results

Judgments of Argument Strength. Our first hypothesis, the prior attitude effect, points to the difficulty people have in putting aside their prior feelings and prejudices when evaluating evidence, even when pro and con arguments have been presented to them in a balanced manner, and even when, as here, Ps are instructed repeatedly to "set their feelings aside," to "rate the arguments fairly," and to be as "objective as possible."

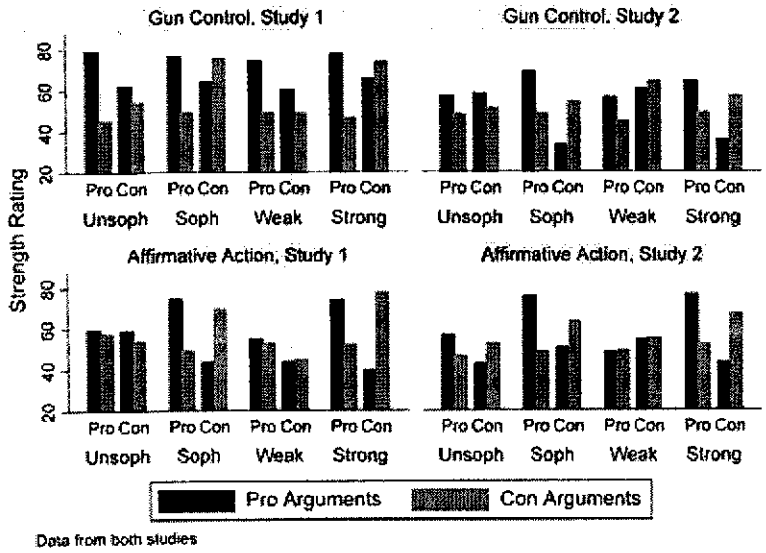
As an initial test of the prior attitude effect (Hypothesis 1), we compare the average strength ratings for pro-attitudinal and counterattitudinal arguments, expecting Ps to rate the congruent stronger than the incongruent arguments. Arguments were rated on a [0,100] scale, with larger values denoting stronger ratings.

Figure 3 displays the results in sets of four bars, broken down by study, issue, sophistication, and strength of prior attitudes. Dark bars represent average strength ratings for pro arguments, light bars con arguments; the first pair of bars shows the responses of proponents of the issue, and the second pair shows responses of opponents. The prior attitude bias is indicated wherever we see higher ratings for congruent than incongruent arguments. Clearly, the prior belief effect is systematic and robust among sophisticates and those who feel the strongest, despite our best efforts to motivate evenhandedness (and despite the fact that across these samples and prior pretest samples, the eight arguments for each issue have statistically equivalent average strength ratings). By contrast with the most knowledgeable and most "crystallized" thirds of our sample, the least sophisticated respondents and those with the weakest prior attitudes on these issues show little or no prior belief effect.

Table 1 reports regression analyses of the impact of prior attitudes on argument strength ratings, with contrasts for the least and most sophisticated thirds of our samples and those with the weakest and strongest priors.[3] Each P's overall rating of the strength of arguments (our dependent variable) was computed as the sum of ratings of the pro arguments minus the sum of ratings of the con arguments, recoded to [0,1]. To test for a prior attitude bias, we regressed these argument strength ratings on attitude extremity at time 1 (as measured by the six-item scale described above, recoded to [0,1]). Significant, positive coefficients support the hypothesis: Ps who favor gun control or affirmative action rate congruent arguments as stronger than incongruent arguments, while those opposed see the con arguments as stronger. Table 1 shows a

[3]Though we believe the display of contrasts in Table 1 presents our results most transparently, the proper tests are interactive. All of the contrasts for affirmative action shown in Table 1, when run as proper interaction models, yield significant results for the interaction term. The interactions for gun control are (obviously) not significant for Study 1, where both sophisticates and nonsophisticates were biased; the sophistication interaction is marginally significant for gun control in Study 2 ($p < .1$), but the attitude strength interaction is not.

FIGURE 3 Argument Strength Ratings, by Sophistication and Strength of Prior



strong prior attitude effect in the predicted direction, with only nonsophisticates and those with weak priors failing to show the effect.

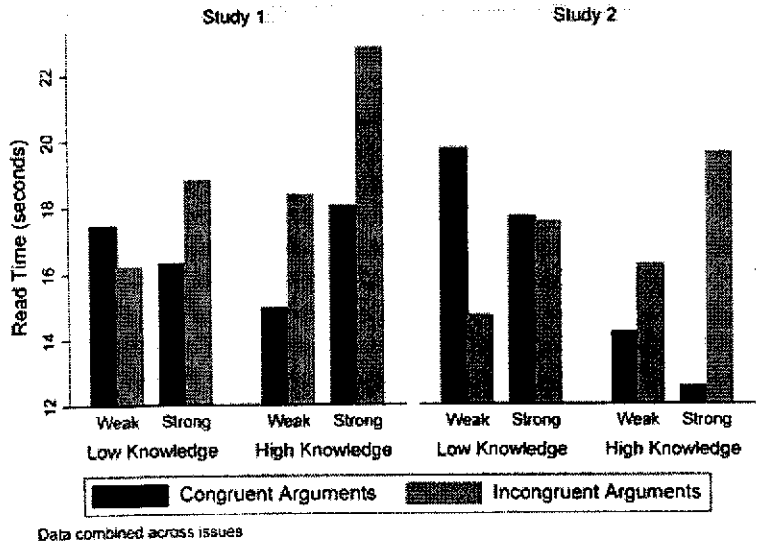
A Disconfirmation Bias. In addition to the prior belief effect, we predict a disconfirmation bias whereby people will too readily accept confirmatory arguments more or less at face value but *actively* counterargue attitudinally incongruent evidence (Hypothesis 2). Moreover, like the prior belief effect we expect this bias to vary with sophistication and strength of prior attitude. Our experimental design allows multiple tests for these predictions. If indeed

TABLE 1 Regressions of Argument Strength Ratings on Prior Attitudes

		All Participants	Least Sophisticated	Most Sophisticated	Weak Priors	Strong Priors
Study 1: Affirmative Action	R^2	.232	.075	.527	.006	.510
	B	**.415(.102)*****	−.234(.212)	**.667(.135)*****	.078(.250)	**.646(.164)*****
	N	57	17	24	19	17
Gun Control	R^2	.302	.390	.535	.054	.335
	B	**.471(.093)*****	**.691(.204)****	**.632(.143)*****	**.479(.154)****	**.537(.161)****
	N	61	20	19	19	24
Study 2: Affirmative Action	R^2	.282	.255	.322	.009	.511
	B	**.381(.075)*****	.257(.172)	**.513(.114)*****	.047(.117)	**.494(.104)*****
	N	67	20	24	20	22
Gun Control	R^2	.195	.023	.333	.084	.220
	B	**.331(.083)*****	.103(.143)	**.477(.151)****	.261(.199)	**.289(.116)***
	N	68	24	22	21	24

Note: This table reports unstandardized coefficients with standard errors in parentheses.
*Significant at the .05 level.
**Significant at the .01 level.
***Significant at the .001 level.

FIGURE 4 Read Times for Argument Strength Ratings



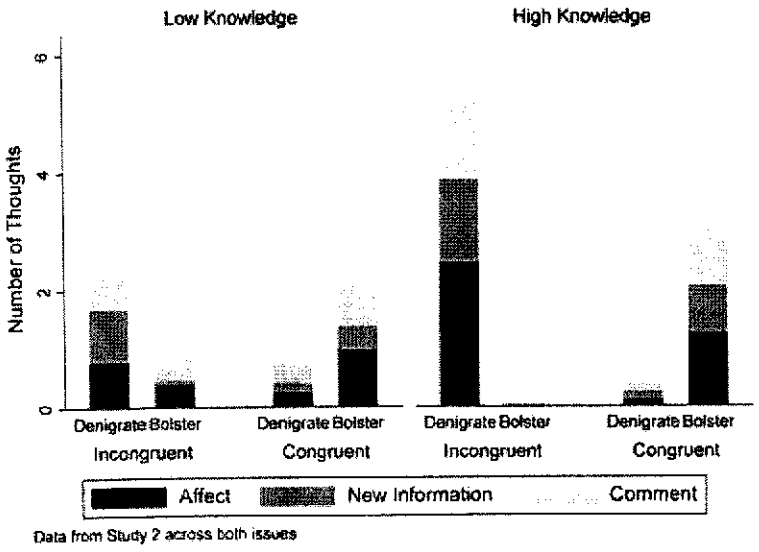
people actively challenge attitudinally incongruent arguments, we would expect them to take more time processing counterattitudinal arguments than pro-attitudinal arguments, and to spend the extra time denigrating, deprecating, and counterarguing the incongruent arguments.

Unbeknownst to the Ps, as they read the eight arguments the computer kept track of the time that elapsed from when they clicked open an argument until they submitted their strength rating. This reading time variable provides an initial test of the disconfirmation bias. Because the pattern of results is the same for both affirmative action and gun control, we show both issues combined in Figure 4, broken down by study to underscore the robustness of the results. For simplicity, and because each study shows virtually the same pattern when taken separately, we report ANOVA analyses for both studies combined. As suggested in Figure 4, Ps in both studies across both issues did take longer to read and process attitudinally challenging arguments, F (1,107) = 3.39, $p = .068$. When averaging across all participants this difference was fairly small (on the order of 1–2 seconds), but the contrast becomes significantly greater for sophisticates and those with stronger prior attitudes (4–7 seconds, or a 25–50% increase in processing time for attitudinally *in*congruent arguments). Indeed, though there were no significant main effects on reading time for sophistication and attitude strength, the interactions of sophistication and strength with argument congruence were highly significant: sophistication * congruence, F (1,107) = 9.96, $p = .002$; attitude strength * congruence, F (1,107) = 4.41, $p = .038$. Finally, it is interesting to note that unsophisticated participants with weak prior attitudes actually spent longer processing congruent arguments, which suggests a confirmatory bias for those participants who lack the resources and motivation to disconfirm challenging arguments.

What were the Ps doing with the extra time spent reading the contrary arguments? To explore this question, we asked participants in Study 2 to list their thoughts for four of the eight arguments they rated, two pro and two con.[4] Our theoretical expectation is that whereas most Ps quickly (and relatively thoughtlessly) assimilate supporting arguments, they more actively process contrary arguments, generating thoughts that denigrate or counter these arguments and bolster their prior convictions. We carried out a direct test of this disconfirmation hypothesis by examining the content of the thoughts Ps listed in response to the two pro and two con arguments for each issue. We coded each thought into one of seven categories (following Edwards and Smith 1996) and then aggregated these codes into three basic response types: *affect*, including general affect for the argument, for the evidence, and for the conclusion; *new information*, including a new fact

[4]Half performed this task immediately, while the other half did so only after completing the posterior attitude items. This allowed us to see whether the act of listing one's thoughts had any significant impact on polarization. It did not, and we pool all thought-listing data.

FIGURE 5 **Mean Number of Thoughts for Congruent and Incongruent Arguments**



Data from Study 2 across both issues

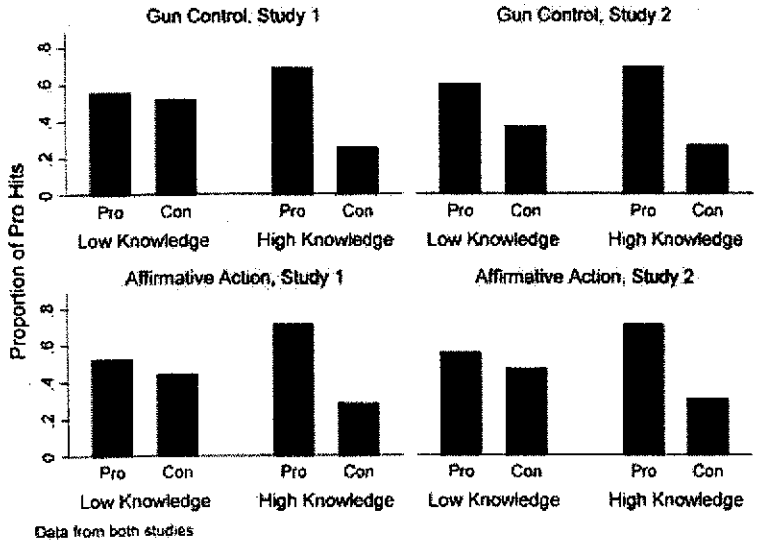
not present in the argument or a new argument; and *comments* about the evidence or about the source. And of course each thought was coded as denigrating or bolstering the presented argument.

Figure 5 depicts these data graphically for both issues combined, breaking down the mean number of thoughts by congruence and sophistication. On average, Ps made 2.5 comments per argument (for a total of 10 thoughts across the four arguments), but there were considerable differences across participants. Perhaps not surprisingly, sophisticated participants produced many more thoughts overall than did their less knowledgeable peers. More interesting, as predicted incongruent arguments elicited far more thoughts than did congruent ones, and these were almost entirely denigrating. Both sophisticated and unsophisticated participants showed this basic pattern of bolstering congruent arguments while denigrating incongruent ones, though sophisticates were clearly more biased. Finally, although we had asked Ps to leave their feelings aside and to concentrate on what made the arguments weak or strong, it is interesting that a goodly number of Ps made simple, content-free affective statements (the darkest portion of each bar), to the effect "I like (don't like) this argument or conclusion" or simply said they liked or disliked the facts or figures supporting an argument. The more demanding types of responses were the introduction of a new fact or an original argument (medium gray) and a comment on the source or quality of the evidence (light gray). In both instances the new evidence brought to mind was overwhelmingly congruent with their priors. Overall, this pattern perfectly conforms to our expectations about disconfirmation.

We performed a mixed-model ANOVA on the number of thoughts generated, with sophistication as a between subjects variable and argument type (congruent or not) and response type (bolster or denigrate) as within subjects variables. The results from this analysis strongly confirm the pattern reported above, with significant main effects for sophistication, $F\ (1,89) = 6.37$, $p = .013$, and argument congruency, $F\ (1,88) = 4.57$, $p = .045$. Moreover, there was a highly significant two-way interaction between argument congruency and response type, $F\ (1,88) = 10.05$, $p = .002$, and a significant three-way interaction between congruency, response type, and sophistication, $F\ (1,88) = 4.07$, $p = .047$, such that sophisticates even more than unsophisticates tend to denigrate incongruent arguments and bolster congruent ones.

A Confirmation Bias. In both experiments, we tested the hypothesis that when given a chance to pick and choose what information to look at—rather than when presented with pro and con arguments—people will actively seek out sympathetic, nonthreatening sources (Hypothesis 3). Both in the "real world" (where Volvo owners read Volvo ads) and in the lab using the information board, citizens can sometimes choose to selectively look or not look

FIGURE 6 **Proportion of Pro-Attitudinal Hits in Free Search**



at information from the opposing side. It bears repeating that this selective exposure hypothesis has met with mixed empirical results in the psychological literature. We believe that this failure to clearly confirm one of the classic expectations of the cognitive dissonance tradition is at least partly due to the affectively tepid issues and arguments that have been used to test it (Edwards and Smith 1996). We expect to find evidence of the confirmation bias with the more contentious and challenging political issues and arguments found in real-world politics.

Recall that in part 1 of both experiments Ps were shown a computerized information board in which each row of a matrix of 16 policy arguments was labeled with a well-known opinion source for the given issue (Figure 2a). As always, instructions were designed to maximize accuracy goals and minimize partisan bias. The most direct measure of bias in search is the proportion of pro-attitudinal hits out of the eight arguments looked at. Figure 6 displays these data graphically by study, issue, and sophistication. For all groups examined, proponents of the issue sought out more supporting than opposing arguments, and this difference was quite substantial for sophisticates in both studies and for both issues. When given the chance, sophisticated respondents selected arguments from like-minded groups 70–75% of the time. For example, on average sophisticated opponents of stricter gun control sought out six arguments of the NRA or the Republican Party and only two arguments from the opposition.

Table 2 presents the results from a regression of this bias measure on t_1 attitude extremity for both studies and both issues. The results are straightforward and confirm the pattern in Figure 6: Ps were more likely to read the argument of a sympathetic source than to expose themselves to an opposing point of view. Supporters of gun control or affirmative action were significantly more likely to search out the arguments of "their" issue groups (e.g., Citizens Against Handguns or the NAACP). As expected, these results are particularly pronounced for sophisticates, where, for example, every 10% increase in support for affirmative action in Study 1 led to a 7.78% increase in the proportion of pro-affirmative action hits on the information board. By contrast, the results for strength of priors were mixed.[5]

As an interesting side note, we also recorded the reading times for Ps in the information board task, expecting a replication of our disconfirmation bias for Ps who did open counterattitudinal arguments. This is what we found. On average across both experiments, Ps spent about 2 seconds longer reading incongruent arguments, with sophisticates spending more than 5 seconds longer when considering an argument from the opposition.

Attitude Polarization. All of these mechanisms—the prior attitude effect, the disconfirmation bias, and the

[5]We also estimated fully interactive regression models to directly test the contrasts in Table 2, finding significant sophistication interactions across the board, but as suggested in Table 2, inconsistent results for the strength of prior attitudes interactions.

TABLE 2 Regressions of Proportion of Pro-Attitudinal Hits on Prior Attitudes

		All Participants	Least Sophisticated	Most Sophisticated	Weak Priors	Strong Priors
Study 1: Affirmative Action	R^2	.106	.114	.605	.003	.162
	B	**.326(.107)***	.338(.284)	**.778(.116)*****	.055(.247)	.402(.161)
	N	54	17	18	18	23
Gun Control	R^2	.130	.029	.352	.002	.481
	B	**.360(.099)****	.170(.171)	**.594(.099)****	.041(.218)	**.693(.106)*****
	N	61	18	24	20	19
Study 2: Affirmative Action	R^2	.107	.051	.520	.059	.151
	B	**.328(.074)****	.226(.080)	**.721(.146)*****	.242(.143)	.389(.137)
	N	69	24	22	24	23
Gun Control	R^2	.313	.164	.505	.293	.249
	B	**.560(.072)*****	.406(.164)	**.711(.089)*****	**.541(.113)****	**.499(.148)***
	N	67	20	24	23	22

Note: This table reports unstandardized coefficients with standard errors in parentheses.
*Significant at the .05 level.
**Significant at the .01 level.
***Significant at the .001 level.

confirmation bias—should theoretically lead to attitude polarization because they deposit more supportive evidence and affect in memory (both in online evaluations and in the associated cognitions that may provide the grist for memory-based processing). Our theory suggests that those on either side of the issues should become more attitudinally extreme in their positions, despite the fact that they were exposed to the same balanced stream of information. As we have already noted, concerted efforts by psychologists to find attitude polarization in bias studies have largely failed when they have used the appropriate direct measures of attitude change.

To test the polarization hypothesis, we regressed t_2 attitude extremity on t_1 extremity. Coefficients significantly greater than 1 indicate polarization (that is, each unit movement on the t_1 attitude scale corresponds to *more* than a unit increase on the t_2 scale).[6] As always, we report contrasts by sophistication and strength of prior attitude; we also consider contrasts of the top and bottom thirds of the sample in degree of bias in the given processing mechanisms. That is, we perform a tertile split on the variables that measure confirmation and disconfirmation biases—the proportion of pro-attitudinal hits in the information board task and the average pro minus average con ratings in the argument strength task, respectively—and contrast the top and bottom thirds.

Pooling the data from both studies (for statistical power), we find strong evidence of attitude polarization for sophisticated participants, those with strong priors, and (most importantly) those who were biased in their information processing. We find polarization across both tasks and both issues (indeed, only one of 12 expected cells in Table 3 fails to achieve significance—strong priors for gun control in the information board task).[7] Looking at the most sophisticated third of the sample who rated affirmative action arguments, for example, the regression slope (1.268) indicates that those with positive priors had even more positive posteriors, while those with negative priors had even more negative posteriors (on average, 27% more extreme). By contrast, unsophisticates and those with weak priors did not polarize (unsophisticates who rated the strength of affirmative action arguments present the one exception to this pattern).

Finally and most important, we find substantial polarization among participants who processed information in a biased manner, but not among those who were less biased. This finding directly and clearly links the processes of motivated skepticism to attitude polarization as our theory predicts, something that previous research

[6] If respondents gave the same responses on the posttest as they did on the pretest, a regression of the form, Posttest $= \beta_0 + \beta_1$ (Pretest) $+ \varepsilon$, would yield $\beta_0 = 0$ and $\beta_1 = 1$. $\beta_1 > 1$ provides evidence of polarization. $0 < \beta_1 < 1$ would show moderation—that is, individuals do not change their opinion, but their attitude on the posttest was weaker than on the first query. Finally, $\beta_1 < 0$ would indicate persuasion—people have changed their opinion on the issue.

[7] As with earlier analyses, fully interactive models confirm the pattern of contrasts shown in Table 3.

TABLE 3 Attitude Polarization, Studies Combined

		Least Sophisticated	Most Sophisticated	Weak Priors	Strong Priors	Least Biased	Most Biased
Argument Strength	R^2	.818	.860	.813	.853	.681	.852
Task: Affirmative	C	−.091(.056)	**−.148(.047)*** **	−.013(.050)	**−.165(.058)*** **	−.021(.068)	**−.137(.052)***
Action	B	**1.195(.090)** **	**1.268(.079)*** **	1.024(.079)	**1.297(.091)*** **	1.072(.114)	**1.237(.082)*** **
	N	41	44	41	37	43	42
Gun Control	R^2	.358	.816	.680	.673	.459	.805
	C	.175(.110)	−.074(.066)	.076(.067)	−.129(.094)	.122(.092)	−.056(.063)
	B	.755(.158)	**1.149(.086)***	.907(.098)	**1.214(.132)***	.805(.146)	**1.164(.086)***
	N	43	41	42	43	38	46
Infoboard Task:	R^2	.716	.912	.770	.870	.680	.888
Affirmative Action	C	.013(.056)	**−.169(.044)*** **	−.044(.060)	**−.107(.041)** **	−.080(.124)	−.055(.049)
	B	.933(.094)	**1.330(.068)*** **	1.097(.097)	**1.177(.068)** **	1.031(.189)	**1.191(.073)** **
	N	41	39	40	47	16	36
Gun Control	R^2	.726	.709	.744	.725	.169	.805
	C	−.045(.070)	**−.133(.082)***	−.076(.072)	−.103(.077)	.274(.321)	**−.168(.067)***
	B	1.044(.153)	**1.223(.121)***	1.142(.140)	1.177(.115)	.626(.492)	**1.277(.094)*** **
	N	42	44	42	42	10	47

Note: This table presents regressions of t_2 attitude extremity on t_1 extremity. Unstandardized coefficients are presented. C indicates the constant. Significance of coefficients is computed relative to a slope of 1.0.
*Significant at the .10 level.
**Significant at the .05 level.
***Significant at the .01 level.

has not been able to do. Those participants whose argument strength ratings were most skewed by disconfirmation biases had significantly more extreme attitudes on affirmative action and gun control after rating the arguments, while those whose ratings were more evenhanded showed no significant attitude polarization. Similarly, confirmation biases—seeking out attitudinally consistent arguments while avoiding inconsistent arguments in the information board—led to more extreme attitudes as compared to the least biased participants for both issues.

In short, despite our best efforts to promote the evenhanded treatment of policy arguments in our studies, we find consistent evidence of directional partisan bias—the prior attitude effect, disconfirmation bias, and confirmation bias—with a substantial attitude polarization as the result. Our participants may have tried to be evenhanded, but they found it impossible to be fair-minded.

General Discussion

Our studies show that people are often unable to escape the pull of their prior attitudes and beliefs, which guide the processing of new information in predictable and sometimes insidious ways. But what does this mean for citizens in a democracy? From one perspective the average citizen would appear to be both cognitively and motivationally incapable of fulfilling the requirements of rational behavior in a democracy. Far from the rational calculator portrayed in enlightenment prose and spatial equations, *homo politicus* would seem to be a creature of simple likes and prejudices that are quite resistant to change. Can this possibly be rational? The normative question, it seems, turns on whether the processing of new information and the updating of one's attitude *needs to be independent of one's priors.*

From one point of view with which we are sympathetic, it can be argued that the attitude strength effect and disconfirmation bias are rational responses to attitude-relevant information; it is perfectly reasonable to give heavy weight to one's own carefully constructed attitudes. This perspective, which would substitute the word "skepticism" wherever "bias" appears in this article, suggests that beliefs and attitudes may be thought of metaphorically as possessions to be protected (Abelson and Prentice 1989). This belief, this feeling, is mine! Like other possessions we paid a purchasing price in terms of time and cognitive resources spent forming and updating our impressions. Many political attitudes, especially those linked to identity (Conover 1988), are worthy of such defense in their own right. To the extent one's attitude reflects considerable prior thought, it may well be more trustworthy than new information, especially if—as is so often the case in the political realm—that new information reflects the strategic behavior of political opponents. Simply put, if one thinks (more pointedly, *feels*) that the veracity of the evidence is dubious, the opposition is wrong, or the media hostile, then why pay them heed?

From another perspective, with which we also have sympathy, Bayesian updating requires independence between priors and new evidence (Evans and Over 1996; Green and Shapiro 1994; but see Gerber and Green 1998). In the extreme, if one distorts new information so that it always supports one's priors, one cannot be rationally responsive to the environment; similarly, manipulating the information stream to avoid any threat to one's priors is no more rational than the proverbial ostrich.

For many citizens, perhaps, the bias may be less extreme, but there are certainly ideologues and bigots who fit both of these descriptions. Luker (1984), for example, found that attitudes among abortion activists are so linked to their beliefs and feelings about sexuality, gender, religion, and family, that they have become completely incapable of entertaining points of view that challenge their own. Sears and Whitney (1973) have found similar stubborn adherence to prior attitudes among those watching a political debate. Our own evidence, presented above, presents a compelling case that motivated biases come to the fore in the processing of political arguments even for nonzealots.

On the other hand and contrary to the intuitions of normative theory (but consistent with the predictions of cognitive psychology), we do find that those with weak and uninformed attitudes show less bias in processing political arguments. This finding may tempt the conclusion that objectivity and tolerance rest more on ignorance and apathy than on the elite skills of ideal citizens. Perhaps we have been looking for rational citizenship in all the wrong places, and it is the great unwashed who save democracy! Provocative though it may be, this interpretation does not stand up to normative, theoretical, or empirical scrutiny. First, we find no empirical evidence of principled moderation among the bottom or middle thirds of our sample, whose extremity scores were statistically indistinguishable from those of the most sophisticated participants. Second, our theory predicts less bias for unsophisticated and uncommitted respondents not because they possess a greater sense of evenhandedness, but rather because they lack the motivation and ability to engage in attitude defense. Finally, this same lack of motivation and knowledge undermines the ability to apply individual preferences to public policy that underlies a normatively secure

democracy, so it would be a dysfunctional objectivity at best.

If we push either side of the rationality argument too strongly we end up playing the clown. So how do we reconcile these positions? Skepticism is valuable and attitudes should have inertia. But skepticism becomes bias when it becomes unreasonably resistant to change and especially when it leads one to avoid information as with the confirmation bias. And polarization seems to us difficult to square with a normatively acceptable model (especially since the supporters and opponents in the policy debate will *diverge* after processing exactly the same information). Moreover, up to some tipping point for persuasion, our model predicts polarization even from unbalanced and counterattitudinal streams of information (see also Rahn, Aldrich, and Borgida 1993; Redlawsk 2001).

How we determine the boundary line between rational skepticism and irrational bias is a critical normative question, but one that empirical research may not be able to address. Research can explore the conditions under which persuasion occurs (as social psychologists have for decades), but it cannot establish the conditions under which it *should* occur. It is, of course, the latter question that needs answering if we are to resolve the controversy over the rationality of motivated reasoning.

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Polit Behav (2014) 36:235–262
DOI 10.1007/s11109-013-9238-0

ORIGINAL PAPER

The Influence of Partisan Motivated Reasoning on Public Opinion

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Published online: 4 July 2013

Abstract Political parties play a vital role in democracies by linking citizens to their representatives. Nonetheless, a longstanding concern is that partisan identification slants decision-making. Citizens may support (oppose) policies that they would otherwise oppose (support) in the absence of an endorsement from a political party—this is due in large part to what is called partisan motivated reasoning where individuals interpret information through the lens of their party commitment. We explore partisan motivated reasoning in a survey experiment focusing on support for an energy law. We identify two politically relevant factors that condition partisan motivated reasoning: (1) an explicit inducement to form an "accurate" opinion, and (2) cross-partisan, but not consensus, bipartisan support for the law. We further provide evidence of how partisan motivated reasoning works psychologically and affects opinion strength. We conclude by discussing the implications of our results for understanding opinion formation and the overall quality of citizens' opinions.

Keywords Motivated reasoning · Parties · Partisan trust · Experiment

Party identification is often seen as playing an important, if not paramount, role when it comes to influencing political attitudes and behaviors. Indeed, few concepts have received more attention among political scientists. Nonetheless, the discipline

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surprisingly continues to lack consensus on *when* partisan identification colors one's interpretation of political information due to a dearth of solid evidence (Bullock et al. 2013; Druckman et al. 2013; Nicholson 2012; Petersen et al. n.d.; Slothuus and de Vreese 2010; Taber and Lodge 2006). In this paper, we explore two fundamental questions. First, when does one's partisan identification slant the evaluation of political information? Our focus here is distinct from prior work, as we focus on an individual's *motivation* at the time of opinion formation (e.g., as opposed to individual sophistication or opinion strength). A second novelty we explore is how the type of partisan endorsement (e.g., same party, different party, bipartisan, what type of bipartisan, etc.) conditions the tendency of individuals to evaluate information through a partisan lens. In addition to exploring these conditions, we also present evidence of how individuals' psychologically process partisan information and thus get at the underlying psychology of partisan motivated reasoning—in some sense this gets at an ongoing debate about how partisan endorsements work in terms of providing a cue to reduce cognitive effort as opposed to coloring how information is interpreted and consciously evaluated (Petersen et al. n.d.). In sum, our focus *differs* from prior work in that we look at whether motivation and partisan sources that almost always connect with policy endorsements condition partisan motivated reasoning.

In the end, our work, when combined with other recent work on the *conditions* under which partisan motivated reasoning occurs, sets the stage for the next generation of research which demands a more full-fledged theory that moves beyond psychological processes and integrates/connects political contexts that drive partisan motivated reasoning. This also would lay the foundation for more serious normative discussions regarding the status of this type of decision-making, as we will discuss in the conclusion.[1]

Partisan Motivated Reasoning

Motivated reasoning refers to an individual's goal in the context of forming an attitude. We follow Taber and Lodge (2006) and focus on two primary motivations in the opinion formation process: *directional* and *accuracy* goals. We begin by discussing the former goal and then turn to the latter. A *directional goal* refers to when a "person is motivated to arrive at a particular conclusion" (Kunda 1999, p. 236), e.g., one that is consistent with a person's party identification (Taber and Lodge 2006; thus we focus here strictly on partisan directional goals). Individuals weigh information consistent with their existing beliefs or social identities more heavily than contradictory information when motivated by a directional goal in

[1] Our work also adds to studies of how party endorsements in general affect public opinion (e.g., Arceneaux 2008; Bullock 2011; Nicholson 2012). Consider, for instance, Bullock's (2011) recent paper, which tests the effects of a partisan endorsement on support for a policy by varying the availability of a source endorsement. He concludes (2011, p. 512), "party cues are influential, but partisans… are generally affected at least as much—and sometimes much more—by exposure to substantial amounts of policy information." What Bullock does not probe deeply, however, is the conditions under which partisan endorsements are likely to slant evaluations.

forming an evaluation (Kunda 1990). Motivated directional reasoning causes people to seek out information that confirms their existing beliefs (i.e., an attitude confirmation bias), counter-argue and dismiss information inconsistent with their existing beliefs regardless of the belief's objective accuracy (i.e., a disconfirmation bias), and view evidence consistent with their prior opinions as stronger, (i.e., a prior attitude effect) (e.g., see Druckman et al. 2013; Kunda 1990, 1999; Lodge and Taber 2000; Slothuus and de Vreese 2010; Taber and Lodge 2006).[2]

Partisan motivated reasoning (i.e., directional goals aimed at protecting one's partisan identification) is likely to occur when one is primed to pay particular attention to being consistent with his/her partisan identity. Partisan identity certainly plays a critical role in public opinion formation and directional reasoning is likely often driven by an individual's desire to be loyal to and consistent with one's own party and maximize differences with the out-party (Lavine et al. 2012; Smith et al. 2005; also see Dancey and Goren 2010, p. 686; Druckman et al. 2013; Green et al. 2002; Iyengar et al. 2012; Nicholson 2012, p. 52). Thus, a Democrat might view a policy sponsored by Democrats as effective and support it, whereas he/she would see the same policy as less effective and oppose it if sponsored by Republicans (and vice versa for Republicans). Druckman et al. (2013) find that party endorsements have a powerful impact on support for off-shore oil drilling in the U.S. and immigration reform in competitive information contexts, and that elite polarization on these issues stimulates partisan motivated reasoning. When individuals engage in motivated reasoning they may miss out on relevant information that might otherwise be helpful (Druckman and Bolsen 2011; Fazio and Olson 2003, p. 149; Jerit 2009; Lavine et al. 2012). This literature leads us to offer the following prediction.

Hypothesis 1 *Individuals will be more likely to engage in partisan motivated reasoning in evaluating a policy when provided with an in-party or out-party endorsement. (This is particularly likely to occur when a directional motivation is at work; without an induced motivation, the hypothesis is less clear.)*

Prior work has identified some factors that moderate the likelihood of partisan motivated reasoning including political sophistication, opinion strength, message repetition, information search, and the level of elite polarization in a given context (e.g., see Taber and Lodge 2006[3]; Bullock 2011; Druckman et al. 2012, 2013). However, one factor that has not been examined by political scientists is an individual's goal in evaluating information in the context of opinion formation (there is sometimes the assumption that directional motivations dominate; see note 4).

[2] Note that motivated reasoning encompasses a range of distinct goals, including defending prior opinions, impression motivation, and behavioral motivation (see Kunda 1999). but here we follow political science work to date focusing on directional and accuracy goals.

[3] These various moderators somewhat contradict Taber and Lodge's (2006, p. 767) conclusion that: "despite our best efforts to promote the even-handed treatment of policy arguments in our studies, we find consistent evidence of directional partisan bias…Our participants may have tried to be evenhanded, but they found it impossible to be fair-minded." Of course even Taber and Lodge themselves find moderating effects of opinion strength and sophistication (also see Druckman 2012).

As intimated, individuals may pursue distinct goals when forming a political opinion. An accuracy goal refers to when individuals are motivated to evaluate information in a manner that will lead to an "accurate" belief or opinion. The goal of forming a *correct* (or "accurate") belief means that an individual will evaluate political arguments with the hope of reaching an outcome that is the "correct or otherwise best conclusion" (Taber and Lodge 2006, p. 756). What the "best outcome" entails is, of course, not always clear. One criterion might be that individuals consider the available information and not ignore potentially relevant arguments in order to form an evaluation consistent with one's partisan identity. This is our focus and it is consistent with the partisan motivated reasoning literature (e.g., Lavine et al. 2012); yet we will discuss how more work is needed on what goals and motivations imply.

As explained, individuals pursue distinct goals in the process of opinion formation. Individuals invest greater cognitive effort in forming an opinion and rely on more complex decision rules when pursuing an accuracy goal (Kunda 1990, p. 485). Measuring someone's commitment to accuracy in the opinion formation process can be difficult given that it is not easy to observe. It is for this reason that psychologists often experimentally induce an accuracy goal (again, something political scientists have not done). For instance, participants in experimental settings pursue an accuracy goal when forming an opinion when they are instructed to consider alternative perspectives and keep in mind that they will have to explain the reasons for their opinions to others (Kunda 1999; Tetlock 1986).[4] In other words, an encouragement to assess how compelling a message is combined with the *anticipation* of having to explain one's opinion generates a motivation to form an accurate opinion (i.e., it vitiates a directional goal). This leads us to offer the following prediction.

[4] This can be accomplished in a variety of other ways, with the underlying rationale being to increase, "the stakes involved in making a wrong judgment or in drawing the wrong conclusion, without increasing the attractiveness of any particular opinion" (Kunda 1990, p. 481). One approach is to inform respondents that their decision is important, will be judged by peers, *will have to be justified*, will be made public, or will affect someone else (also see, e.g., Tetlock 1983; Tetlock et al. 1989; Lerner and Tetlock 1999; Tetlock 1986, all of whom do not explicitly look at social expectations but use it as a clear implicit component of their treatments).

As will become clear, we follow this approach (i.e., inducing participants to believe they will have to justify their responses). This approach differs from the one taken by Taber and Lodge (2006, p. 759), who ask respondents to, "view information in an evenhanded way so [as to] explain the issue to other students." The potential problem with not asking explicitly for general justification is highlighted by Lord et al. (1984) who find that inducing people to form accurate preferences requires not only encouraging them to be unbiased, *but also inducing them to justify their opinion*. Taber and Lodge's manipulation asks respondents to put their prior opinions aside and requires them to "explain the issue" to others. However, individuals may have understood this to mean that they need to present some facts to others; they may not have been induced to consider alternative viewpoints or justify their opinions. This is why we follow this other experimental work by asking respondents to justify their specific opinions (e.g., Redlawsk 2002; Tetlock 1983). Indeed, Houston and Fazio (1989, p. 65) explain that removing attitudinal slant requires "directing people to focus on the nature of the judgmental process" (also see Creyer et al. 1990; Lerner and Tetlock 1999).

Hypothesis 2 *Individuals will be less likely to engage in partisan motivated reasoning when pursuing an accuracy goal in the opinion formation process, regardless of any partisan endorsement.*

As will become clear when we describe the experiment we conducted, we compare an accuracy motivation inducement to no motivation inducement, as well as to a directional motivation inducement in order to offer two points of comparison. Our rationale for doing so will be discussed in the design section.

Another individual level variable known to moderate partisan motivated reasoning is the strength of one's partisanship. Indeed, this is a theme of Lavine et al.'s (2012) book that offers compelling evidence that those more ambivalent about their partisan identity, all else constant, engage in less partisan motivated reasoning. We explore another, albeit possibly related, moderator to Lavine et al. (2012) by measuring trust in one's party (rather than ambivalence). We report these results in Appendix 2.

Cross-Partisan Versus Consensus Bipartisan Sponsorship

As discussed, scholars have only recently begun to explore moderators of partisan motivated reasoning. Surprisingly, only a few studies have explored how the nature of the elite partisan environment affects opinion formation and this focus has been only on polarized environments (e.g., Druckman et al. 2013). Yet, the reality is that there is a fair deal of policy passed that is enacted with bipartisan support (see, e.g., Harbridge 2013).

There are at least two alternative ways parties can play a role in endorsing policies, aside from strictly along partisan lines. First, there is what some Congressional scholars refer to as a *cross-partisan* environment—that is, an environment where a policy is supported by a mix of members from different parties (Cooper and Young 1997). In other words, segments of each party—but not everyone in both parties—vote together. Such cases signal *intra*-party disagreement, which may vitiate partisan motivated reasoning by alerting citizens to conflict within one's own party on an issue. Such conflict has been shown to generate deeper thought regarding the applicability of various pieces of political information (e.g., Chong and Druckman 2007).

Clearly, this differs from a polarized context where nearly all members of each party vote together, and, more importantly, it differs from what we refer to as *consensus* bipartisan sponsorship where nearly all members of both parties support a policy. In other words, there is an important distinction between these contexts—strictly partisan (see Hypothesis 1), *cross-partisan* and *consensus* bipartisan support since the former introduces conflict into what is often deemed a unified group (i.e., a political party), and the latter will likely lead individuals to simply see their party as supportive (along with the other party). This leads us to make the following prediction.

Hypothesis 3 *Individuals will be less likely to engage in partisan motivated reasoning when they are provided with a cross-partisan endorsement in which some, but not all, members of a partisan's party are described as supporting a*

policy, even when provided with a directional inducement (since the conflict may generate elaboration).[5]

As stated, we expect Hypothesis 3 to hold regardless of whether there is a directional processing inducement present in any given context (see Hypothesis 1). Again, the logic is that the intra-party conflict vitiates one's partisan identity and undermines partisan motivated reasoning. As mentioned, this is quite distinct from a consensus bipartisan situation where nearly all members of both parties support a policy (see Slothuus and de Vreese 2010). As stated, individuals may focus on the fact that their party supports the policy, thereby increasing the likelihood of partisan motivated reasoning.[6]

Hypothesis 4 *Individuals will be more likely to engage in partisan motivated reasoning when they are provided with a consensus bipartisan endorsement.*

Note that we expect Hypothesis 4 will *not* hold when individuals pursue an accuracy goal in the process of forming an opinion. Our expectations accentuate the conditional nature of partisan motivated reasoning. Rather than being an inevitable political decision-making outcome, its occurrence depends both on one's motivation in forming an opinion and the nature of partisan support.

Processing Party Endorsements

Thus far we have focused on the conditions under which partisan endorsements will (or will not) slant the evaluation of political information. As explained, we have built explicitly on a theory of partisan motivated reasoning, putting aside another debate: *how do partisan endorsements affect individuals' opinions?* For some, partisan effects operate as a perceptual screen (Campbell et al. 1960). However, for others partisan endorsements are akin to a heuristic where an individual may simply follow the endorsement and ignore the content of a policy or political argument (Downs 1957). This debate has become coined a "motivated reasoning" versus "cue theory" debate—and although the language may be a bit distinct from the intellectual origins (given cues or heuristics have their origins in psychology as a type of bias and not simply skipping over content *per se*, see Druckman et al. 2009b), the more important point for us is whether people are in fact using party endorsements as a way to expend less cognitive effort when asked to evaluate political information, or whether they see the endorsement and use that to process information more thoroughly a la partisan motivated reasoning.

This distinction in process is best captured by Petersen et al. (n.d., p. 3):

> Research on the psychology of opinion formation suggests that two different psychological processes may explain citizens' reliance on a source cue such as

[5] We are careful here because such an endorsement could work to generate something akin to an accuracy goal given that conflict can generate elaboration (e.g., Chong and Druckman 2007) which is what we posit; however, it also is possible that the endorsement just leads to a moderation of opinions. We thank an anonymous reviewer for this point.

[6] We thank Laurel Harbridge for pointing out the important distinction between unanimity and cross-partisan situations.

> a party's position on a policy. The first process, heuristic processing, minimizes the processing costs involved in opinion formation while the second process, motivated directional reasoning (or for short, motivated reasoning), involves investing cognitive effort to defend valued pre-commitments such as one's party identification (e.g., by spending effort to produce convincing arguments for giving into the motivational pull of one's identification)... While a few studies have suggested that motivated reasoning drives the processing of party cues...Bullock (2011, p. 497) sums up the current state of the literature by arguing that party cues are widely thought to be processed heuristically. Yet, until now, studies on party cues in political science have not focused directly on the psychological processing of party cues and, hence, have failed to discern between the different possibilities. This is unfortunate because the two processes are grounded in different types of motivations and paint very different pictures of citizens' basic relation to politics... If party sponsor effects originate in heuristic processing, citizens are basically motivated to hold accurate opinions... and partisan bias in opinion formation is just an unfortunate by-product of citizens' lack of political interest... In contrast, if party sponsor effects originate in motivated reasoning, citizens are seen as motivated to be biased....

Thus, on one hand, a party endorsement may lead individuals to ignore information at stake in a policy debate as a way to expend less cognitive effort. On the other hand, it may motivate effortful processing of relevant information as a way to protect one's partisan identity. Petersen et al. (n.d.) report evidence that adding party endorsements to policy arguments lengthens individuals' processing time, concluding that motivated reasoning drives the process—that is, people do not skip over the information but rather use the endorsement as a perceptual screen leading them to think in even more elaborate ways. In short, the idea is that partisan motivated reasoning causes people to take longer to form an opinion because people think through the substance of the argument and its source rather than merely skipping the substance and following the source endorsement.

We follow the lead of these authors and explore *how partisan endorsements shape opinions* by comparing response latency times in the presence and absence of a partisan endorsement. We expand upon the method introduced by Petersen et al. (n.d.) by exploring response latency to see if party endorsements lengthen the time participants spent forming an opinion, as they expect and find. This feeds into an ongoing debate, reviewed in detail by Petersen et al. (n.d.) about whether party endorsements are followed blindly as simple cues or actually enhance effortful opinion formation processes but in a potentially skewed manner.

Hypothesis 5 *The amount of time it will take individuals to form an opinion will increase in the presence of a partisan endorsement if, in fact, partisan motivated reasoning is driving opinion formation (and individuals are not simply following a party endorsement as a way to reduce cognitive effort).*

A final hypothesis concerns what might happen if partisan motivated reasoning is at work. Specifically, when individuals engage in partisan motivated reasoning, their

goal is to affirm an opinion they already hold (Taber and Lodge 2006). In this sense, individuals may view new information as bolstering their prior opinion, and such added evidence may boost the certainty—and, consequently, the strength—of their opinion (Atkeson and Maestas 2012; Druckman and Leeper 2012; Druckman and Bolsen 2011). In contrast, people will sort through evidence that they see as going in different directions when they are motivated by an accuracy goal. This may stunt attitude strength and, hence, people may attach less importance to their opinion. Along these lines is research by Brader (2006, Chaps. 4–5) who reports decreases in opinion strength when individuals are anxious, an affective state that prompts information acquisition (and has been shown elsewhere to limit motivated reasoning; see Atkeson and Maestas 2012).

Hypothesis 6 *Individuals' will express greater strength in an opinion if it is formed via partisan motivated reasoning.*

Experiment

We tested our hypotheses with a survey experiment in August 2010. We used the Internet to draw a sample that was representative of the U.S. population.[7] A total of 1,600 respondents took part. We opted to focus on opinions about an energy policy for a number of reasons. First, it is clearly a salient issue area of increasing importance. Second, few studies to date explore the dynamics of opinions about energy policy (for a review, see Bolsen and Cook 2008). Third, both parties offered support for various energy propositions, which was a necessary element if we are to test varying party endorsements. For example, the *Energy Policy Act of 2005* was originally sponsored by three Republicans: Representatives Joe Barton [R-TX6] (primary sponsor), Richard Pombo [R-CA11], and William Thomas [R-CA22], but received wide Democratic support. The *Energy Independence and Security Act of 2007* was sponsored by Democratic Representative Nick Rahall of West Virginia, but it ended up being signed into law by Republican President George W. Bush.[8] In the House, 96 Republicans voted "no," 95 voted "yes," and 10 did not vote; while

[7] We contracted with a survey research company (Bovitz Inc.) to collect the data. The sample was drawn from a panel of respondents who have opted into complete online surveys. The panel was originally developed based on a random-digit-dial (RDD) telephone survey, where to enter the panel a respondent needed to have access to the Internet. (In this sense, it is a non-probability sample in the same way as those taken by firms such as YouGov are non-probability samples.) The panel has continued to grow based on ongoing RDD recruiting and referrals. From the panel, which has ~1 million members, a given sample is drawn using a matching algorithm to ensure that those screened to qualify for the survey constitute a sample that demographically represents the United States.

[8] To explore the possibility of extra-ordinary pre-treatment effects, we content analyzed news articles from *The New York Times* and *The USA Today* from June 2008 to approximately June 2009 that included one of the following terms in the headline or lead paragraph: "energy policy," "energy crisis," "energy shortage," or "energy plan." From these, we selected articles that met specific criteria to ensure they are about the U.S. energy situation. This resulted in a total of 67 articles (28 from the *USA Today* and 39 from the *NYT*). We found that 39 % mentioned some type of partisan content (from one party) and 6 % mentioned some sort of bipartisanship. These results suggest nothing out of the norm a la pre-treatment and that partisanship plays a role in these discussions.

219 Democrats voted "yes," 4 voted "no," and 9 did not vote. In the Senate, it was a total of 86 "yes" votes to 8 "no" votes, and, as mentioned, a Republican President signed the bill into law. Thus, we can credibly and honestly attribute the Energy Act of 2007 to either party or to both. We decided to focus on the 2007 Act for this reason.

In order to construct a baseline condition, we needed to isolate portions of the Act that would not automatically trigger partisan motivated reasoning. While the 2007 Act had various attributes, we focused on three central tenants that, via pre-tests, we found did not signal a partisan slant in one direction or another.[9] We told all respondents:

We are next going to ask you what you think about parts of the 2007 Energy Independence Act. The Act included the following provisions:

- Requires U.S. automakers to boost gas mileage to 35 miles per gallon for all passenger cars by 2020, which is a 40 % increase.
- Funds for research and development of solar and geothermal energy, and for the increased production of biofuels.
- Provides small businesses loans toward energy efficiency improvements.

This is the only information control group participants received, followed by the measures we describe below. We opted for these elements because, as confirmed by our aforementioned pre-test, it was brief enough that people could recall the information, and it includes elements that may be construed as traditionally more liberal (e.g., funds for alternative energies) or conservative (e.g., small business loans). Before discussing our measures, we first describe how we manipulated processing motivation and endorsements.

Design

We randomly assigned participants to one of three motivational conditions: no motivation (in which case, nothing was added to the above description), a directional motivation condition (in which case, a partisan directional goal was induced), and an accuracy motivation condition (in which case, an accuracy goal was induced). We opted for three motivational conditions because, as explained, a partisan directional motivation is the inverse of an accuracy motivation, and thus, a useful point of comparison.

Operationally, we followed the conventional approach within psychology for inducing an accuracy motivation by asking participants to consider multiple perspectives and telling them they would later have to justify the reasons for their judgment (e.g., Kunda 1999; Tetlock 1986; also see note above on our approach). Specifically, the introduction prior to the bullet points concerning the Act read:

> We are next going to ask you what you think about parts of the 2007 Energy Independence Act. **When thinking about your opinion, please try to view**

[9] We asked pre-test respondents whether they thought the Act was sponsored by Democrats or Republicans, and we found no significant differences in presumed attributions.

> **the policy in an evenhanded way and from various perspectives. We will later ask that you justify the reasons for your judgment – that is, why the policy's content is more or less appealing.** The Act included the following provisions:...

The bolded portion highlights the motivated reasoning manipulation. It is bolded here for presentational purposes and was not bolded in the original survey. Note that we did, in fact, later ask for such justification.

There is much less prior research to which we can turn to for guidance when it comes to inducing a directional motivation in the opinion formation process as most prior work focuses on inducing an accuracy goal.[10] Thus, we induced respondents to justify why they affiliate with a party as a way to motivate the defense of one's partisan identity prior to evaluating the energy law. Specifically, the introduction prior to the bullet points concerning the Act read:

> We are next going to ask you what you think about parts of the 2007 Energy Independence Act. **When thinking about your opinion, please consider the bill was passed during a period of divided government where fellow partisans voted together nearly 90 % of the time. This was necessary to ensure coherent policy programs. We will later ask you about your party and why you affiliate with it (or why you choose to not affiliate with a party).** The Act included the following provisions:...

We again bolded the manipulation here, although this was not done in the actual survey; also, we did, in fact, later ask participants why they affiliate with a party. The directional manipulation puts an emphasis on defending one's partisanship and accentuates partisan identity.[11] Emphasizing the importance of coherent partisanship also makes clear that party identification matters in this context.

We also randomly assigned participants to one of five partisan endorsement conditions. The partisan endorsement always came after the processing manipulation; for example, the accuracy motivation treatment followed by a Democratic Party endorsement read (with the endorsement manipulation in bold; again, it was not bolded in the survey):

> We are next going to ask you what you think about parts of the 2007 Energy Independence Act. When thinking about your opinion, please try to view the policy in an evenhanded way and from various perspectives. We will later ask that you justify the reasons for your judgment – that is, why the policy's content is more or less appealing. **The Energy Act, overall, was widely**

[10] Personal communication, Charles Taber 12/28/09, and personal communication Milton Lodge 12/31/09. The closest example we could find was Boiney et al. (1997, p. 8) who ask respondents to decide whether to introduce a new product for a company with a directional manipulation telling them that the product is profitable and that past proposals have been turned down too quickly. We build on this general approach. Redlawsk (2002) manipulates motivation in a study of motivated reasoning, but focuses on on-line versus memory-based processing; he assumes on-line is the default, and then manipulates memory-based processing by telling people they will have to list everything they can remember and justify their choice. This latter aspect will likely prompt more accuracy processing, which is what Redlawsk (2002) wants to show—i.e., that memory-based processing moderates motivated reasoning.

[11] We thank Charles Taber for suggesting this specific approach; personal communication, 1/4/10.

supported by Democratic representatives and included the following provisions:...

The Republican endorsement was identical, but instead of saying "Democratic" it said "Republican." Again, this statement is true if one focuses on the final vote margin in the Senate and President Bush signing the bill into law. For a Democrat, the Republican endorsement would be the "different" party condition (and vice versa).

The consensus bipartisan endorsement replaced "... was widely supported by [Democratic/Republican] representatives" with "was widely supported by representatives from both parties..." The idea here, as motivated by Hypothesis 4, is that the Act has the full support of both political parties; thus, we anticipate partisan motivated reasoning in the presence of a consensus bipartisan endorsement when directional motivated reasoning is induced. This differs from Hypothesis 3 which predicts that introducing intra-party conflict/disagreement will decrease the likelihood of partisan motivated reasoning. The cross-partisan bipartisan endorsement stated that the Energy Act "was supported by some, but not all, representatives, of both parties..." The idea here is to make clear that members within each party were divided.

Table 1 displays the conditions to which respondents were randomly assigned. For the "same" and "different" party conditions, we simply matched people's self-reported partisan identification (measure described below) with the party endorsement in the condition to which participants were randomly assigned. Table 1 also lists predictions based on our hypotheses relative to the baseline we use to evaluate whether partisan motivated reasoning slants opinions—i.e., no partisan endorsement with an accuracy reasoning motivation (Condition 3, Table 1). As mentioned, we include an accuracy inducement as part of the baseline condition since we suspect that in the absence of such an inducement motivated reasoning may occur if, as mentioned, directional reasoning is the default method for forming evaluations in political contexts. We recognize that this is a high standard, but we believe it provides a normatively compelling baseline (see Druckman 2012). Not only is it obtainable, but it also enjoys a number of other advantages over alternative approaches. This standard addresses Schattschneider's (1960, p. 132) concern that "the most disastrous shortcomings of the system have been those of the intellectuals whose concepts of democracy have been amazingly rigid..." (As we will note below, our results are robust to using a baseline that includes no accuracy inducement.)

For the "no motivation" conditions displayed in Table 1, we do not include predictions, instead writing "depends," by which we mean the impact of any endorsement is contingent on whether the "norm" is to pursue a directional or accuracy goal in the absence of an experimental inducement toward one of these motivations. We also do not offer predictions in the conditions in which no endorsement is provided in the context of a directional or accuracy motivational inducement (see Table 1), because it is unclear how individuals will respond when not given a partisan endorsement to anchor evaluations *per se*.

Table 1 Experimental conditions and predictions

	No endorsement	Same party endorsement	Different party endorsement	Consensus endorsement	Cross-partisan endorsement
No motivation	*Condition 1* Control/ baseline	*Condition 4*	*Condition 7*	*Condition 10*	*Condition 13*
	?	Depends	Depends	Depends	No change (Hyp. 3)
Directional motivation	*Condition 2*	*Condition 5*	*Condition 8*	*Condition 11*	*Condition 14*
	?	Increase support (Hyp. 1)	Decrease support (Hyp. 1)	Increase support (Hyp. 4)	No change (Hyp. 3)
Accuracy motivation	*Condition 3*	*Condition 6*	*Condition 9*	*Condition 12*	*Condition 15*
	Baseline	No change (Hyp. 2)	No change (Hyp. 2)	No change (Hyp. 2)	No change (Hyp. 2 & 3)

Measures

We included appropriate measures to test each of our hypotheses, as well as a number of other variables shown in prior work to influence attitudes toward energy policies. We discuss the control measures and models that include these variables as robustness checks in Appendix 1 (Tables 5, 6); all of the main treatment effects we report below are robust to the inclusion of the full set of control variables.

We measured party identification with a standard, fully-labeled, 7-point measure that asked, "Generally speaking, which of the options on the scale below best describes your party identification?" where 1 = "strong Democrat" and 7 = "strong Republican." Like other studies of partisan attitudes, we group leaning partisans with partisans because they tend to behave similarly (e.g., Baum and Groeling 2009; Bullock 2011; Clarke and Stewart 1998; Dennis 1992; Druckman 2001; Druckman et al. 2012; Keith et al. 1992; Levendusky 2010; Magleby et al. 2011; Petrocik 1974; 2009). We also follow these studies by excluding individuals who identify as a pure Independent. In our case—as with many other Internet samples—the percentage of respondents who identify as an Independent is larger than that found in the National Election Studies. This seems to be an unexplained dynamic found in most web-based surveys (e.g., Chang and Krosnick 2009; Malhotra and Krosnick 2007). Our total N is 1,070 once we exclude pure Independents with 56.5 % identifying with the Democratic Party and 43.5 % with the Republican Party.

Our central dependent variable is straightforward and was asked immediately after exposure to an information treatment (see above). Respondents were asked: "Given this information, to what extent do you oppose or support the Energy Act?" on a 7-point fully labeled scale ranging from 1 = "strongly oppose" to 7 = "strongly support." The mean score is 4.97 (Std. Dev. = 1.66). This is the same type of dependent variable used in prior studies of partisan motivated reasoning such as Taber and Lodge (2006).

Table 2 Support for the 2007 Energy Act by Condition

	No endorsement	Same party	Different party	Consensus endorsement	Cross-partisan endorsement
Scores by condition					
No motivation	*(Condition 1)*	*(Condition 4)*	*(Condition 7)*	*(Condition 10)*	*(Condition 13)*
	Mean: 5.21	5.30	4.07	5.37	4.89
	(Std. Dev.: 1.78)	(1.39)	(1.79)	(1.59)	(1.41)
	90 % CI: (4.87, 5.56); $N = 73$	(5.03, 5.57); $N = 73$	(3.74, 4.40); $N = 80$	(5.04, 5.70); $N = 64$	(4.60, 5.18); $N = 66$
Directional motivation	*(Condition 2)*	*(Condition 5)*	*(Condition 8)*	*(Condition 11)*	*(Condition 14)*
	5.24	5.74	3.96	5.50	5.05
	(1.65)	(1.23)	(1.59)	(1.37)	(1.39)
	(4.93, 5.56); $N = 77$	(5.51, 5.97); $N = 79$	(3.65, 4.26); $N = 76$	(5.25, 5.77); $N = 77$	(4.78, 5.34); $N = 68$
Accuracy motivation	*(Condition 3)*	*(Condition 6)*	*(Condition 9)*	*(Condition 12)*	*(Condition 15)*
	4.76	4.59	4.90	5.02	4.78
	(1.54)	(1.99)	(1.49)	(1.78)	(1.79)
	(4.44, 5.08); $N = 65$	(4.17, 5.02); $N = 62$	(4.62, 5.19); $N = 76$	(4.68, 5.37); $N = 73$	(4.40, 5.17); $N = 61$

Entries in each cell report the mean support for the 2007 Energy Act (1–7 oppose/support scale), standard deviation in parentheses, 90 % confidence interval associated with estimated support in parentheses, and the *N*. Baseline condition is in boldface

We measured the response time it took for individuals to answer this question to explore how partisan endorsements shape opinions and test Hypothesis 5. We follow Mulligan et al.'s (2003) suggestion of analyzing response times using a Cox proportional hazard model with logged response times (more on this below). We also added a conventional measure to assess opinion strength regarding support for the Act. Respondents were asked, immediately after the question about support for the Energy Act, "How important to you is your opinion towards the Energy Act (e.g., how strongly do you feel about your opinion)?" on a 7-point, fully labeled scale ranging from 1 = "extremely unimportant" to 7 = "extremely important."

Results

We begin by reporting the impact of the experimental conditions on support for the *2007 Energy Act*. We report, in Table 2, the mean support for the Energy Act, standard deviation, 90 % confidence interval, and *N* for each condition.[12] The results are perhaps easier to interpret with a figure demonstrating changes in support for the Energy Act across experimental conditions.

We plot, in Fig. 1, the change in support for the *2007 Energy Act* for each condition relative to the baseline (i.e., no endorsement, accuracy reasoning

[12] We use one-tailed tests throughout as is conventional given clear directional predictions; see Blalock 1979; hence our 90 % confidence intervals.

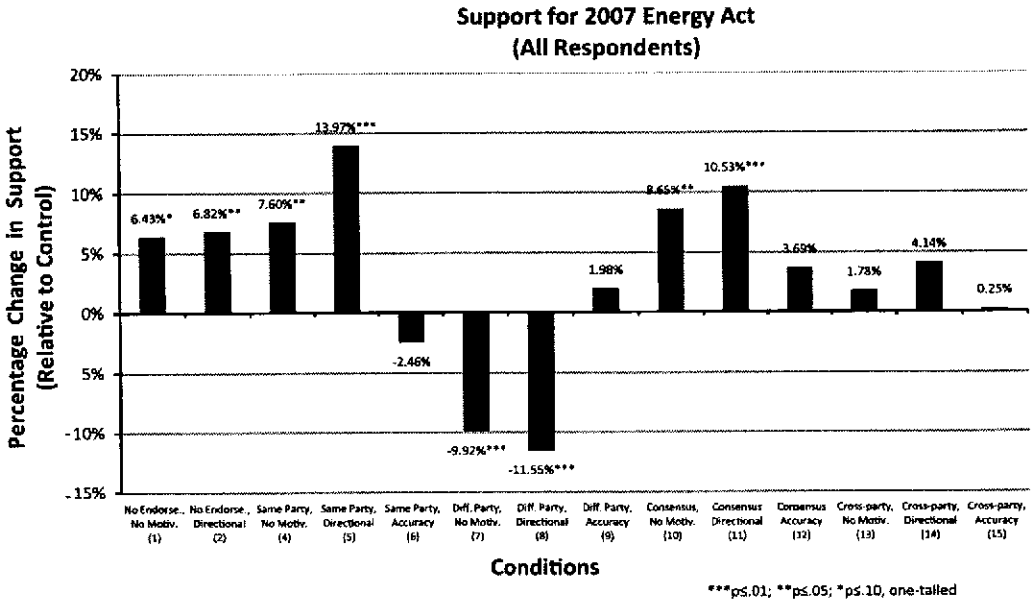

Fig. 1 Support for the 2007 Energy Act

motivation, see Table 1). Figure 1 displays the percentage change in the likelihood of support for the Act for each condition relative to the baseline's mean score of 4.76 on the 7-point response scale. All of the conditions that are significant in Fig. 1 remain significant when we estimate support for the Energy Act using an ordered probit model which includes additional control measures, as reported in Table 5 of Appendix 1.[13]

The first notable result is the strong support for Hypothesis 1. When individuals are primed to defend their partisan identity, they shift their evaluations of the Act toward the position endorsed by their own party when those positions are provided (+13.97 % vs. the control, Condition 5, Fig. 1), and away from positions endorsed by the other party when those positions are provided (−11.55 % vs. the control, Condition 8, Fig. 1). Thus, there is clear evidence of motivated reasoning in the conditions where a partisan endorsement is provided and a directional motivation is induced.[14] The shifts in support for the Act are smaller in magnitude in the absence of a directional inducement (+7.60 and −9.92 % vs. the control), but in the same direction (see Conditions 4 and 7, respectively, Fig. 1). Thus, while we did not directly hypothesize that the no motivation conditions would resemble the directional conditions, this seems to be the case—sans an accuracy inducement on this issue, partisan motivated reasoning occurs.

There is also clear support for Hypothesis 2 which predicted that a motivation to form an accurate, or correct, opinion would eliminate partisan motivated reasoning. There is no significant difference in support for the Energy Act relative to the baseline (see Conditions 6, 9, 12, and 15, Fig. 1) in every case where we induce an

[13] The question wording and distribution of each response for all control variables is reported in Table 6.

[14] Note that moving in the opposite direction of an out-party endorsement is consistent with others who find a similar backlash effect (Cohen 2003; Redlawsk 2002).

accuracy motivation, regardless of whether a partisan endorsement is present. Clearly, when individuals are induced to hold "correct" views (i.e., justify their opinions), partisan motivated reasoning does not slant opinions.

Cross-Partisan Versus Consensus Bipartisan Endorsements

We find strong support for Hypothesis 3, which focuses on the effect of a cross-partisan endorsement on support for the Energy Act. In the cross-partisan endorsement (Conditions 13 and 14, Fig. 1), partisan motivated reasoning disappears (i.e., support for the Energy Act in these conditions does not differ significantly from the baseline). This supports other recent work which clearly shows that political conditions can eliminate partisan motivated reasoning by introducing conflict that stimulates elaboration (Druckman et al. 2013). We also find strong support for Hypothesis 4. Individuals' support for the Act significantly increases in the presence of a consensus bipartisan endorsement (Conditions 10 and 11) (of course when accuracy is induced, there is no effect, see Condition 12). Indeed, as expected, support for the Act in the consensus bipartisan conditions resembles the increase in support found in the directional processing, same party condition (Condition 5; likewise compare Conditions 4 and 10 to see partisan motivated reasoning in the absence of motivational prompts). People ostensibly hear that everyone in their party supports a policy, and, even though their party is joined in support of that policy by the opposition party, they still support it more than they otherwise would sans an endorsement. Thus, a consensus bipartisan endorsement does not decrease the likelihood of partisan motivated reasoning; rather, its effect on opinions is in line with that of a same party endorsement.[15]

How Partisan Motivated Reasoning Works and Opinion Strength

We next report how long it took respondents to answer our primary dependent measure asking about support for the Energy Act. As explained, analyzing response latency enables us to probe the psychology underlying partisan motivated reasoning. If it works as a perceptual screen (as opposed to an opportunity to skip over substantive information), we would see longer response times in the conditions in which a partisan endorsement is present (see Hypothesis 5). On the other hand, if participants are using the endorsements as a way to ignore other information, processing times should become *shorter* in the conditions where a partisan

[15] Note that the directional processing motivation Conditions (2, 5, 8, 11, and 14) significantly exceeded the no manipulation processing Conditions (1, 4, 7, 10, and 13) in only one of five cases. The one case is the same party endorsement, no motivation relative to same party endorsement, directional motivation conditions (Conditions 4 and 5, $p < 0.05$). The no endorsement conditions with no processing manipulation (1) and a directional processing inducement (2), perhaps surprisingly, register significant increases in support for the policy. Interestingly, the increase in support in these conditions stems entirely from movement among Democrats (evidence on this is available upon request from the authors). In short, in the absence of any processing inducement, Democrats seem to engage in motivated reasoning to a greater extent than Republicans when they are induced to think about and justify their views. This presumably reflects that energy is an issue owned by Democrats (see Druckman et al. 2009a).

endorsement is present. The time measured is the point at which the question appeared to the point at which an answer to the question is provided (in milliseconds).

We follow Mulligan et al.'s (2003) suggestion of analyzing response latency times using a Cox proportional hazard model. This is a type of survival model that explores the time that passes before an event occurs (i.e., the answer to a question); however, it reports coefficients that represent a hazard rate and thus higher coefficients indicate the question was answered more quickly. We also follow others in using a logged measure of response latency—specifically logged milliseconds (Huckfeldt et al. 1999; Mulligan et al. 2003, p. 273); however, our results are consistent if we use ranked response times (e.g., Petersen et al. n.d.) or the untransformed data.

We are confident that our results are not influenced by extreme outliers. The use of logs limits outlier effects. Moreover, the company we contracted with cut off extremely slow responders, further limiting the influence of outliers. Finally, our results are robust even if we eliminate some of the remaining longer times. Also note that while small changes in logged milliseconds may seem trivial to the naked eye they are difficult to interpret straightforwardly given the logged response times. More importantly, in survey responses even these small changes can suggest powerful implicit processes (see Chugh 2004; Petersen et al. n.d.).

Table 3 reports the results of two separate models. Recall that higher coefficients suggest a failure to spend as much time answering the question. Model 1 displays the results with all conditions included except for the baseline, which is the same as in all prior analyses—i.e., no endorsement, accuracy motivation. The results presented in Model 1 show that the accuracy motivation (except for Condition 6) and cross-partisan bipartisan conditions did significantly increase processing time (as we expected); however, with one exception (Condition 10), the other conditions were answered more slowly suggesting thoughtful/elaborative processes underlying partisan motivated reasoning.

Model 2 in Table 3 includes only the directional reasoning and single party/consensus endorsement conditions in order to test whether adding a party endorsement increases processing time relative to the pure control group baseline (i.e., here the baseline is Condition 1). There is clear evidence, in support of Hypothesis 5, that adding a party endorsement increases processing time significantly. The other no endorsement Condition (2) is not significantly different from the baseline, as one would expect, but five of the other six conditions in which a same party, different party, or consensus endorsement is present significantly increases the time participants spent answering the dependent measure. Thus, people do not appear to be using partisan endorsements as a way to avoid effortful cognition (i.e., as a heuristic processing shortcut), but, are instead basing their evaluations in part on their own partisan identity. In short, the presence of an endorsement significantly increases processing time (Conditions 5, 7, 8, 10, and 11, Model 2, Table 3), which indicates more cognitive effort is being expended by participants in these conditions. In sum, the results suggest that motivated reasoning is driving the observed impact of partisan endorsements on policy evaluations—this supports Petersen et al's (n.d.) contention that partisan sponsorship colors one's interpretation of the substance of political information rather than serving as a means to avoid effortful cognition.

Table 3 Proportional hazard model using log time (response latency, milliseconds)

Condition	Hazard ratio (standard error) model 1	Hazard ratio (standard error) model 2
No endorse./no motiv. (1)	2.12 (.36)***	–
No endorse./directional (2)	1.94 (.32)***	.91 (.14)
Same party/no Motiv. (4)	1.82 (.31)***	.85 (.14)
Same party/directional (5)	1.59 (.26)***	.75 (.12)**
Same party/accuracy (6)	1.00 (.17)**	–
Diff. party/no motiv. (7)	1.28 (.21)*	.61 (.09)***
Diff. party/directional (8)	1.31 (.22)*	.62 (.10)***
Diff. party/accuracy (9)	.97 (.16)	–
Consensus/no motiv. (10)	1.20 (.21)	.56 (.09)***
Consensus/directional (11)	1.41 (.23)**	.65 (.10)***
Consensus/accuracy (12)	1.11 (.19)	–
Cross-party/no motiv. (13)	1.10 (19)	–
Cross-party/directional (14)	1.00 (.17)	–
Cross-party/accuracy (15)	1.16 (.20)	–
Log-likelihood/*N*	−6368.11/1,070	−3225.47/599

Condition numbers correspond to those listed in Table 1. Standard deviations are listed in parentheses. Baseline condition in model 1 is Condition 3 (see Table 1). Baseline condition in model 2 is Condition 1

* $p < .1$; ** $p < .05$; *** $p < .01$(one-tailed tests)

To test Hypothesis 6, we measured opinion strength. We did so to see if those who engage in motivated reasoning when forming their opinion express increased strength in the opinion. Table 4 reports the results from an ordered probit estimation of the importance individuals attach to their opinions regarding support for the Energy Act with a dichotomous measure included for each experimental condition. There is strong support for Hypothesis 6. Participants in all of the conditions in which support for the Act was shown to be based on partisan motivated reasoning—i.e. all same party, different party, and consensus bipartisan endorsement conditions (except Condition 10)—reported significantly greater importance associated with their opinion toward the Energy Act. This effect is largest in the different party endorsement (7 and 8) and consensus endorsement Conditions (10 and 11). We believe this could have substantial downstream consequences as it makes people less persuadable, less flexible, and more dogmatic (see Druckman 2012; Lavine et al. 2012 for detailed normative discussion).

Conclusion

Partisan motivated reasoning depends on individual characteristics and elite partisan circumstances. We find clear evidence of partisan motivated reasoning when we provided endorsements from either an in- or out- party. For instance, Democrats and Republicans are significantly *more supportive* of the Energy Act when it is endorsed

Table 4 Perceived importance of the 2007 Energy Act

Condition	Coefficient (standard error)
No endorse./no motiv. (1)	.06 (.17)
No endorse./directional (2)	.22 (.17)
Same party/no motiv. (4)	.26 (.17)*
Same party/directional (5)	.26 (.17)*
Same party/accuracy (6)	.30 (.18)*
Diff. party/no motiv. (7)	.42 (.17)***
Diff. party/directional (8)	.34 (.17)**
Diff. party/accuracy (9)	−.15 (.17)
Consensus/no motiv. (10)	.41 (.18)***
Consensus/directional (11)	.39 (.17)***
Consensus/accuracy (12)	.22 (.17)
Cross-party/no motiv. (13)	.16 (.17)
Cross-party/directional (14)	−.15 (.17)
Cross-party/accuracy (15)	−.01 (.18)
Log-likelihood/*N*	−1764.52/1,070

Entries are ordered probit coefficients with standard errors in parentheses

* $p < .1$; ** $p < .05$; *** $p < .01$ (one-tailed tests)

by in-group partisan elites, but significantly *less supportive* of the same policy when it is endorsed by out-group partisan elites. On the other hand, partisan motivated reasoning disappeared when we either induced people to form an accurate opinion or when there was a cross-partisan bipartisan endorsement. Our results additionally provide suggestive evidence that partisan motivated reasoning works as a perceptual screen—i.e., people read and interpret the information in an effortful manner and do not simply follow the endorsement as a way to avoid thinking.

Where does this leave us when it comes to understanding partisanship and its effects on public opinion formation? The last 5–10 years has seen a renascence of work on partisan motivated reasoning. As mentioned, scholars have now moved beyond identifying its occurrence to isolating moderators including individual level factors such as sophistication and opinion strength, message repetition, information search, and partisan polarization. To this, we added what we consider to be two critical aspects of the reality of politics—the *source of political information and the motivation underlying individuals' opinion formation process*. From here, we believe it is time scholars move beyond testing moderators and/or documenting the presence of partisan motivated reasoning and work towards a more complete theory of partisan motivated reasoning in political contexts. Indeed, as far as we know only Gerber et al. (2010) have definitively shown that partisanship causes certain behaviors. A fuller theory clearly will involve considering the relative impact of individual level variables (e.g., we did not find effects for sophistication), context, and source. We believe the motivation driving opinion formation clearly matters and this has been a topic lacking in study.

It may be that political scientists instead focusing on the content or basis of opinions (e.g., how much knowledge, ideological constraint), may be best off looking at motivation. For example, issue publics may be motivated on some issues

and not others (e.g., Bolsen and Leeper n.d.). Another primary source of prompting accuracy motivation may be the social context (e.g., Sinclair 2012). Indeed, if one anticipates having to explain and justify oneself (akin to the manipulation we employed) in a social setting, it may generate an accuracy motivation. But this is where things can get complicated. If that social group is a mixed group, then it may be akin to our accuracy motivation that focused largely on generating a focus on substance. However, if that group is comprised of all in-party strong partisans, an accuracy motivation may lose out to a directional motivation (e.g., Druckman et al. 2013). The role of groups and their relationship to motivation in the opinion formation process seems like an area ripe for future work.[16] More generally, unpacking motivation requires a mix of a consideration of material incentives (e.g., Prior and Lupia 2008; Bullock et al. 2013) in addition to social ones, as well as the potential for hybrid goals and alternative goals besides accuracy and directional.

Finally, there are several intriguing/vexing normative implications of partisan motivated reasoning. There is a lack of consensus among scholars as to what constitutes a normatively appealing opinion (see Druckman 2012). Our results will be troubling for people who worry that partisan motivated reasoning leads to lower quality opinions due to dogmatism and inflexibility (e.g., Lavine et al. 2012). However one could also make the case that relying on one's partisanship (e.g., a partisan directional goal) in the face of limited policy information is "smarter" than trying to assess the policy's content oneself (see Druckman et al. 2012 for further discussion). The bottom line is that our results only further highlight the lack of consensus on what a quality opinion is and the need for a much more detailed discussion and exchange on this topic between empirical and normative scholars—perhaps the focus should shift from considering the informational basis or ideological nature of opinions to the motivation underlying the opinion formation process, but this raises questions about unpacking the determinants of an accuracy motivation, as discussed above. The issue on which we focused—energy policy—deserves a final word. Energy policy, as mentioned, is a topic that has received scant attention among public opinion scholars. Given the future challenges of long-term sustainability, we see this issue area as one in need of much greater exploration as a topic itself.

Acknowledgments We thank Aries Arugay, Kevin Levay, Julia Valdes, and especially Josh Robison for research assistance. We also are grateful to Matt Baum and Sean Richey for insightful comments and the Initiative for Sustainability and Energy at Northwestern (ISEN) for research funding.

[16] There also is the question about whether partisan motivated reasoning leads to polarization. Elite partisan polarization itself appears to increase motivated reasoning (e.g., Druckman et al. 2013; Levendusky 2010; Slothuus and de Vreese 2010). In other words, partisan reasoning will be most likely to occur on issues where the parties conflict or are most dissimilar. We cannot directly examine this because we look at a single case at a single point in time, and, thus, there is no objective variation in polarization. We also do not manipulate polarization (perceptions) as Levendusky (2010) and Druckman et al. (2013) do. We did, however, measure perceptions of partisan similarity. Specifically, we asked "In general, to what extent do you think Democrats and Republicans take similar or dissimilar policy positions?" on a 1–7 scale with higher scores indicating greater similarity. Although we do not report the results from this analysis here (these are available upon request from the authors), we find clear evidence that partisan motivated reasoning occurs to a greater extent among those who view the parties as most different on this issue. This provides further evidence suggesting that partisan motivated reasoning can exacerbate polarization.

Appendix 1

Table 5 offers a robustness check by adding controls for models we used to estimate treatment effects. We include the following categories of variables (see the precise wording for each measure in Table 6).

1. *Demographics and control variables.* We include standard demographics that tend to influence political attitudes: gender (*Female*), minority status (*Minority*), *age*, *education*, partisanship, income, trust, and knowledge in different domains (i.e., *political knowledge*, *energy knowledge*, and *science knowledge*). We do not have clear directional predictions for these variables so we use two-tailed tests for statistical significance. We also measure media exposure with the idea that any coverage may have been positive in terms of the need to address energy problems.
2. *Values/Ideology.* We measure political *ideology* with the idea that conservatives will be less supportive of the Act due to increased government regulation. We also include two worldview variables of communitarian (*EqRgtsToofar*) and egalitarianism (*GovOut*) (Kahan et al. 2009). We expect those who are more individualistic (anti-communitarian) and hierarchical (anti-egalitarian) will be less supportive of the law as they tend to put more faith in market solutions. We include a variable capturing the extent to which the economy is favored over the environment (*EconEnv*).
   We also added a question that asked about the extent to which individuals believe in the "precautionary principle" that is: "When it comes to decisions about energy production, do you think the guiding principle should be whether there will be harm to the environment and/or the public?" (i.e., *Precaution).*
3. *Attitudes about government's role when it comes to energy policy.* We included an item that measures the extent to which the government is the cause of an energy problem (*CauseGov*). The more people view government as a cause, the less likely they may be to see government as the solution. We also ask explicitly about whether government is responsible for addressing the nation's energy problem (*RespGov*), which should correlate with increased support since this is a government law. We include an item that rates the extent to which laws are a good way to address energy issues (*ApphConsum*). Finally, we measured trust in government to specifically address energy problem (*TrustUSGov*). This is interesting because it allows us to see if a domain specific trust in government measure is more appropriate than the aforementioned general trust in government item (*TrustGov*) in explaining support for the Energy Act. Note that the two variables are correlated at about .61, but this does not present a problem in terms of estimating a model with both variables included.

We recognize that several Conditions (e.g., 6, 9, 12) offer similar predictions and thus we could merge these dichotomous variables in the regression (via interactions). If we were to do so the results would be robust/unchanged. We opted to not do this simply because we offered condition by condition predictions in Table 1 and thus believe the approach we employ is the most straightforward.

Table 5 Determinants of support for 2007 Energy Act

Variable	Coefficient (standard error)
No endorse./no motiv. (1)	.29 (.18)*
No endorse./directional (2)	.38 (.17)**
Same party/no motiv. (4)	.37 (.17)**
Same party/directional (5)	.73 (.17)***
Same Party/accuracy (6)	−.08 (.18)
Diff. party/no motiv. (7)	−.56 (.17)***
Diff. party/directional (8)	−.58 (.17)***
Diff. party/accuracy (9)	.04 (.17)
Consensus/no motiv. (10)	.46 (.18)**
Consensus/directional (11)	.38 (.17)**
Consensus/accuracy (12)	.08 (.17)
Cross-party/no motiv. (13)	−.00 (.18)
Cross-party/directional (14)	.04 (.18)
Cross-party/accuracy (15)	.01 (.18)
Female	.01 (.07)
Minority	−.05 (.08)
Age	.00 (.00)
Education	.01 (.03)
Income	−.00 (.03)
Media	.18 (.13)
PIDrep	−.04 (.02)**
Ideology	.03 (.02)
EqRgtsToofar	−.03 (.01)**
GovOut	−.04 (.02)**
EconEnv	−.01 (.02)
Precaution	.11 (.03)***
TrustGov	.02 (.06)
TrustUSGov	.05 (.02)**
Political knowledge	.06 (.12)
Energy knowledge	.26 (.13)**
Science knowledge	.05 (.11)
CauseGov	−.04 (.02)*
RespGov	.07 (.02)***
ApphConsum	.12 (.02)***
Log-likelihood/*N*	−1743.67/1,070

Entries are ordered probit coefficients with standard errors in parentheses

* $p < .1$; ** $p < .05$; *** $p < .01$ (one-tailed tests)

Appendix 2: Party Trust as a Moderator of Partisan Motivated Reasoning

Lavine et al. (2012) find that the ambivalence of one's partisan identity moderates partisan motivated reasoning. This is sensible because when one feels a strong attachment to one's party, he/she is more likely to reason in ways that defend and

Table 6 Descriptive statistics for control variables and names of condition variables

Variable	Question/distribution	Mean/ (std. dev.)
Female	Are you male (50 %) or female (50 %)	N/A
Minority	Which of the following do you consider to be your primary racial or ethnic group? (white = 73 %; minority = 27 %)	N/A
Age	What is your age?	45.47 (16.33)
Education	What is the highest level of education you have completed? (1 = less than high school (1 %); 2 = high school (16 %); 3 = some college (39 %); 4 = 4 year college degree (31 %); 5 = advanced degree (13 %))	N/A
Income	Estimate of family income (before taxes) 1 = < $30,000 (24 %); 2 = $30,000–$69,999 (42 %); 3 = $70,000–$99,999 (19 %); 4 = $100,000–$200,000 (13 %); 5 = > 200,000 (2 %)	2.24 (1.02)
Media	How often do you obtain energy information from… newspapers, TV, online (0–1 scale, alpha = .54)	N/A
PIDrep	Generally speaking, do you consider yourself a Democrat, Independent, or Republican? (1 = strong Democrat (16 %); 2 = weak Democrat (9 %); 3 = lean Democrat (14 %); 4 = Independent (33 %); 5 = lean Republican (12 %); 6 = weak Republican (6 %); 7 = strong Republican (12 %))	N/A
Ideology	Which point on this scale best describes your political views? very liberal (1) = 6 %; mostly liberal (2) = 10 %; somewhat liberal (3) = 11 %; moderate (4) = 36 %; somewhat conservative (5) = 14 %; mostly conservative (6) = 13 %; very conservative = 9 %	N/A
EqRgtsToofar	Agreement with "We have gone too far in pushing equal rights in this country." (1 = strongly disagree (21 %); 2 = moderately disagree (9 %); 3 = slightly disagree (9 %); 4 = neither disagree nor agree (19 %); 5 = slightly agree (16 %); 6 = moderately agree (11 %); 7 = strongly agree (16 %))	3.95 (2.07)
GovOut	Agreement with "If the government spent less time trying to fix everyone's problem, we'd all be a lot better off." (1 = strongly disagree (6 %); 2 = moderately disagree (6 %); 3 = slightly disagree (7 %); 4 = neither disagree nor agree (19 %); 5 = slightly agree (17 %); 6 = moderately agree (16 %); 7 = strongly agree (28 %))	4.96 (1.82)
EconEnv	More important to "protect the environment" or "maintain prosperous economy"? (1 = definitely protect environment (8 %); 2 = very likely protect environment (9 %); 3 = probably protect environment (10 %); 4 = equally important (43 %); 5 = probably maintain prosperous economy (13 %); 6 = very likely maintain prosperous economy (10 %); 7 = definitely maintain prosperous economy (7 %))	4.03 (1.55)
Precaution	When it comes to decisions about energy production, do you think the guiding principle should be whether there will be harm to the environment and/or the public?	N/A
	Definitely should not be the guiding principle (1) = 2 %; should play a limited role (2) = 10 %; not sure (3) = 11 %; should play an important role (4) = 46 %; definitely should be the guiding principle (5) = 31 %	
TrustGov	How much of the time do you think you can trust the government in Washington to do what is right? (4 = just about always (2 %); 3 = most of the time (18 %); 2 = only some of the time (61 %); 1 = never (20 %))	2.06 (0.66)

Table 6 continued

Variable	Question/distribution	Mean/ (std. dev.)
TrustUSGov	Rate how much *trust* you have in each source to deal with the nation's energy problems… [U.S. government]. None at all (1) = 19 %; not very much (2) = 23 %; a little (3) = 19 %; a moderate amount (4) = 19 %; a good amount (5) = 11 %; a great deal (6) = 7 %; complete trust (7) = 2 %	3.11 (1.60)
Political knowledge	Know majority required to over-ride veto (56 % correct)	.68 (.32)
	Know which party has majority in U.S. House = (72 % correct)	
	Know whose responsibility it is to declare law unconstitutional = (76 % correct)	
	Know current U.S. Sec. of State = (67 % correct)	
Energy knowledge	Know the world's largest exporter of oil = (63 % correct)	.50 (.29)
	Know renewable energy sources = (63 % correct)	
	Know most U.S. oil not imported from ME = (24 %)	
Science knowledge	Is it true or false that lasers work by focusing sound waves?	.64 (.34)
	Which travels faster: light or sound?	
	0 correct = 14 %; 1 correct = 44 %; 2 correct = 42 %	
CauseGov	Listed below are different sources people tend to mention when they think about the *causes* of the nation's energy problems. To the extent that there are problems, rate how responsible you think each source is for *causing* the U.S.'s energy problems… [U.S. government] not responsible at all (1) = 2 %; not very responsible (2) = 3 %; a little responsible (3) = 8 %; moderately responsible (4) = 14 %; responsible a good amount (5) = 20 %; very responsible (6) = 37 %; completely responsible (7) = 17 %	5.26 (1.38)
RespGov	Listed below are different sources people tend to see as responsible for *addressing (or fixing)* the energy situation. Rate how responsible you think each source is for *dealing with* the U.S.'s energy problems… [U.S. government] not responsible at all (1) = 3 %; not very responsible (2) = 3 %; a little responsible (3) = 5 %; moderately responsible (4) = 13 %; responsible a good amount (5) = 18 %; very responsible (6) = 38 %; completely responsible (7) = 21 %	5.35 (1.46)
ApphConsum	Response to "Do you think the success of energy policy depends on whether individual citizens take actions that reduce energy demand?" (1 = not at all (2 %); 2 = not much (3 %); 3 = a little (7 %); 4 = somewhat (14 %); 5 = a good amount (29 %); 6 = a great deal (29 %); 7 = completely depends (17 %))	5.18 (1.37)

cohere with his/her attachment (Bullock 2011; Cohen 2003). As that attachment weakens, the motivation to defend it may as well. Lavine et al. (2012) argue that those with weaker partisan attachments are less likely to engage in partisan motivated reasoning. They state (2012, p. 122): "partisan strength [i.e., ambivalence for them] …undercuts the judgmental confidence that citizens typically derive from partisan cues, [and] they should turn away from these perceptual anchors and pay

more attention to the particulars. As a result, they should hold more accurate perceptions…".

We did not include a measure analogous to theirs but we did measure a somewhat related measure of party attachment—trust in one's party (see Visser et al. 2006 on attitude strength). Specifically, we asked "To what extent do you trust members of your political party to provide good advice about which energy policies to support?" on a 7-point, fully labeled scale ranging from 1 = "not at all" to 7 = "completely." We opted for this domain specific trust measure given that people's evaluation of a party often varies across issue domains, and we are interested in the strength of people's attachment in the domain of energy.[17]

The bottom line is we find strong support for the argument that trust in one's party moderates the effects stemming from partisan motivated reasoning. We display the results in Figs. 2 and 3, which are analogous to Fig. 1 except that Fig. 2 focuses on respondents with low trust ($N = 538$) and Fig. 3 looks only at those with high trust ($N = 506$). The figures show an enormous moderating effect of trust in one's party on partisan motivated reasoning in evaluating the Energy Act. For those with relatively weak attachments to their partisan identity (Fig. 2), with one exception, there is evidence of partisan motivated reasoning *only* when there is an explicit directional motivation prompt and a partisan endorsement is present. The one exception is a significant effect in the other party, no motivation condition (Condition 7, Fig. 2). In cases of significance, the effects are smaller than in the "all respondent" data (Fig. 1). In short, individuals with weaker attachments to their partisan identity clearly engage in less partisan motivated reasoning.

The treatment effects among individuals with a stronger attachment to their partisan identification Fig. 3 show a much more pervasive influence stemming from the presence of a partisan endorsement. There are very large treatment effects in all of the directional motivation, party endorsement conditions—i.e., same party (Condition 5), other party (Condition 8), and consensus endorsement (Condition 11). However, as predicted, in the presence of a cross-partisan endorsement (Conditions 13, 14, and 15) the effect disappears. Also, in the accuracy manipulation conditions, as predicted, there continues to be no significant partisan motivated reasoning. In sum, those who are less trusting of their party are less likely to engage in motivated reasoning and do so only when explicitly prompted to defend/think about their partisan identity. On the other hand, those who have relatively higher levels of trust in their partisan identity are significantly more likely to engage in motivated reasoning when there is a partisan endorsement present. We ran interactions for cases where both low and high trust groups registered significant effects to explore differences between low- (Fig. 2) and high- (Fig. 3) trust individuals within the same condition. The results show, in every case, the differences are statistically significant (available upon request from the authors).

[17] We employ a median split for this measure which allows us to focus on what are more likely to be qualitatively distinct groups (as do Druckman and Nelson 2003; Miller and Krosnick 2000, p. 305). We find consistent results, albeit slightly weaker, using a continuous measure rather than using a median split. Of note, we find our party trust measure does not moderate support for the Act in conditions where a party endorsement was not provided, as one would expect given trust should only moderate support for the Act in cases where partisan motivated reasoning occurs.

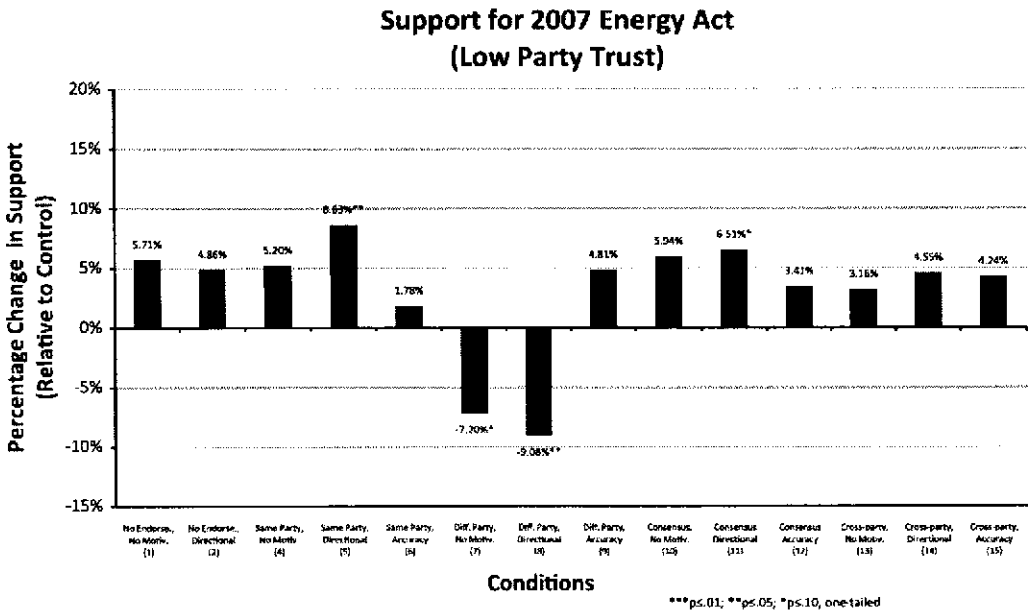

Fig. 2 Support for the 2007 Energy Act

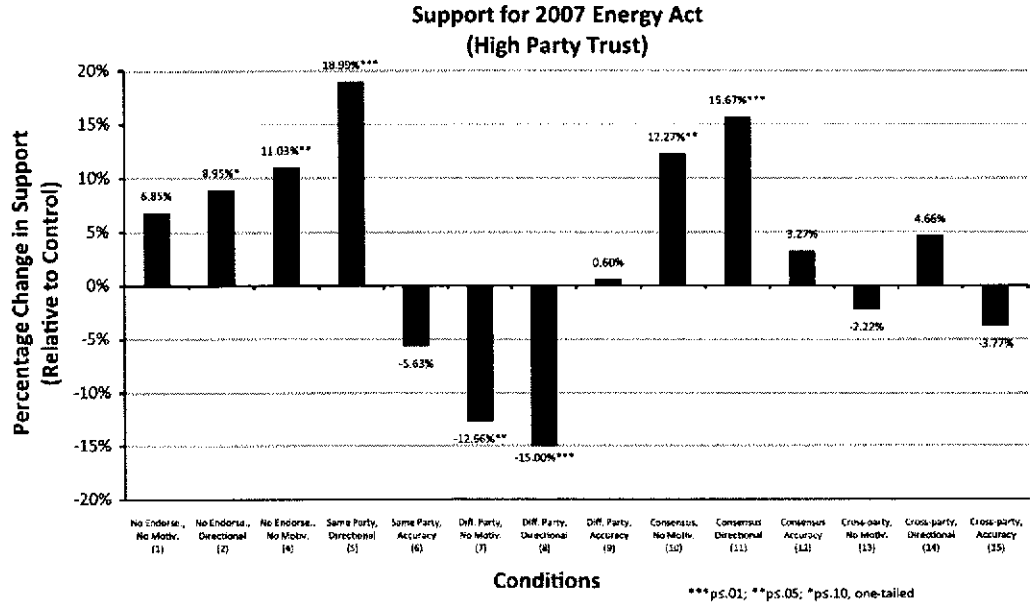

Fig. 3 Support for the 2007 Energy Act (high trust)

This is all quite interesting because we find results similar to Lavine et al. (2012) but using a distinct measure, speaking to the need for more work on moderators of motivated reasoning, starting with a direct comparison between the effectiveness of distinct constructs found to serve as a moderator.

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